ANNUAL REPORT

OF

THE BOARD OF REGENTS

OF THE

SMITHSONIAN INSTITUTION,

SHOWING THE

OPERATIONS, EXPENDITURES, AND CONDITION OF THE INSTITUTION FOR THE YEAR 1864.

WASHINGTON:
GOVERNMENT PRINTING OFFICE.
1865.

LETTER

OF THE

SECRETARY OF THE SMITHSONIAN INSTITUTION,

COMMUNICATING

THE ANNUAL REPORT OF THE OPERATIONS, EXPENDITURES, AND CONDITION OF THE INSTITUTION FOR THE YEAR 1864.

SMITHSONIAN INSTITUTION,
Washington, March 1, 1865.

In behalf of the Board of Regents, I have the honor to submit to the Congress of the United States the annual report of the operations, expenditures, and condition of the Smithsonian Institution for the year 1864.

I have the honor to be, very respectfully, your obedient servant,

JOSEPH HENRY,
Secretary Smithsonian Institution.

Hon. H. HAMLIN,
President of the Senate.

Hon. S. COLFAX,
Speaker of the House of Representatives.

ANNUAL REPORT OF THE BOARD OF REGENTS

OF THE

SMITHSONIAN INSTITUTION,

SHOWING

THE OPERATIONS, EXPENDITURES, AND CONDITION OF THE INSTITUTION UP TO JANUARY, 1865, AND THE PROCEEDINGS OF THE BOARD UP TO MARCH 1, 1865.

To the Senate and House of Representatives:

In obedience to the act of Congress of August 10, 1846, establishing the Smithsonian Institution, the undersigned, in behalf of the Regents, submit to Congress, as a report of the operations, expenditures, and condition of the Institution, the following documents:

1. The Annual Report of the Secretary, giving an account of the operations of the Institution during the year 1864.
2. Report of the Executive Committee, giving a general statement of the Smithsonian fund, and also an account of the expenditures for the year 1864.
3. Proceedings of the Board of Regents up to March, 1865.
4. Appendix.

Respectfully submitted:

S. P. CHASE, *Chancellor.*
JOSEPH HENRY, *Secretary.*

OFFICERS OF THE SMITHSONIAN INSTITUTION.

MARCH, 1865.

ABRAHAM LINCOLN, *ex officio* Presiding Officer of the Institution.
SALMON P. CHASE, Chancellor of the Institution.

JOSEPH HENRY, Secretary of the Institution.
SPENCER F. BAIRD, Assistant Secretary.
W. W. SEATON, Treasurer.
WILLIAM J. RHEES, Chief Clerk.

A. D. BACHE,
RICHARD WALLACH,
RICHARD DELAFIELD,
} Executive Committee.

REGENTS OF THE INSTITUTION.

H. HAMLIN, Vice-President of the United States.
S. P. CHASE, Chief Justice of the United States.
R. WALLACH, Mayor of the City of Washington.
L. TRUMBULL, member of the Senate of the United States.
GARRETT DAVIS, member of the Senate of the United States.
——— ——— member of the Senate of the United States.
S. S. COX, member of the House of Representatives.
J. W. PATTERSON, member of the House of Representatives.
H. W. DAVIS, member of the House of Representatives.
W. B. ASTOR, citizen of New York.
T. D. WOOLSEY, citizen of Connecticut.
L. AGASSIZ, citizen of Massachusetts.
A. D. BACHE, citizen of Washington.
RICHARD DELAFIELD, citizen of Washington.

MEMBERS EX OFFICIO OF THE INSTITUTION.

ABRAHAM LINCOLN, President of the United States.
HANNIBAL HAMLIN, Vice-President of the United States.
W. H. SEWARD, Secretary of State.
W. P. FESSENDEN, Secretary of the Treasury.
E. M. STANTON, Secretary of War.
G. WELLES, Secretary of the Navy.
WM. DENNISON, Postmaster General.
E. BATES, Attorney General.
S. P. CHASE, Chief Justice of the United States.
D. P. HOLLOWAY, Commissioner of Patents.
RICHARD WALLACH, Mayor of the City of Washington.

HONORARY MEMBER.

J. P. USHER, Secretary of the Interior, (*ex officio.*)

PROGRAMME OF ORGANIZATION

OF THE

SMITHSONIAN INSTITUTION.

[PRESENTED IN THE FIRST ANNUAL REPORT OF THE SECRETARY, AND ADOPTED BY THE BOARD OF REGENTS, DECEMBER 13, 1847.]

INTRODUCTION.

General considerations which should serve as a guide in adopting a Plan of Organization.

1. WILL OF SMITHSON. The property is bequeathed to the United States of America, "to found at Washington, under the name of the SMITHSONIAN INSTITUTION, an establishment for the increase and diffusion of knowledge among men."

2. The bequest is for the benefit of mankind. The government of the United States is merely a trustee to carry out the design of the testator.

3. The Institution is not a national establishment, as is frequently supposed, but the establishment of an individual, and is to bear and perpetuate his name.

4. The objects of the Institution are, 1st, to increase, and 2d, to diffuse knowledge among men.

5. These two objects should not be confounded with one another. The first is to enlarge the existing stock of knowledge by the addition of new truths; and the second, to disseminate knowledge, thus increased, among men.

6. The will makes no restriction in favor of any particular kind of knowledge; hence all branches are entitled to a share of attention.

7. Knowledge can be increased by different methods of facilitating and promoting the discovery of new truths; and can be most extensively diffused among men by means of the press.

8. To effect the greatest amount of good, the organization should be such as to enable the Institution to produce results, in the way of increasing and diffusing knowledge, which cannot be produced either at all or so efficiently by the existing institutions in our country.

9. The organization should also be such as can be adopted provisionally; can be easily reduced to practice, receive modifications, or be abandoned, in whole or in part, without a sacrifice of the funds.

10. In order to compensate, in some measure, for the loss of time occasioned by the delay of eight years in establishing the Institution, a considerable portion of the interest which has accrued should be added to the principal.

11. In proportion to the wide field of knowledge to be cultivated, the funds are small. Economy should, therefore, be consulted in the construction of the building; and not only the first cost of the edifice should be considered, but also the continual expense of keeping it in repair, and of the support of the establishment necessarily connected with it. There should also be but few individuals permanently supported by the Institution.

12. The plan and dimensions of the building should be determined by the plan of the organization, and not the converse.

13. It should be recollected that mankind in general are to be benefited by the bequest, and that, therefore, all unnecessary expenditure on local objects would be a perversion of the trust.

14. Besides the foregoing considerations, deduced immediately from the will of Smithson, regard must be had to certain requirements of the act of Congress establishing the Institution. These are, a library, a museum, and a gallery of art, with a building on a liberal scale to contain them.

SECTION I.

Plan of Organization of the Institution in accordance with the foregoing deductions from the will of Smithson.

TO INCREASE KNOWLEDGE. It is proposed—

1. To stimulate men of talent to make original researches, by offering suitable rewards for memoirs containing new truths; and,
2. To appropriate annually a portion of the income for particular researches, under the direction of suitable persons.

TO DIFFUSE KNOWLEDGE. It is proposed—

1. To publish a series of periodical reports on the progress of the different branches of knowledge; and,
2. To publish occasionally separate treatises on subjects of general interest.

DETAILS OF THE PLAN TO INCREASE KNOWLEDGE.

I. *By stimulating researches.*

1. Facilities afforded for the production of original memoirs on all branches of knowledge.
2. The memoirs thus obtained to be published in a series of volumes, in a quarto form, and entitled Smithsonian Contributions to Knowledge.
3. No memoir on subjects of physical science to be accepted for

publication which does not furnish a positive addition to human knowledge, resting on original research; and all unverified speculations to be rejected.

4. Each memoir presented to the Institution to be submitted for examination to a commission of persons of reputation for learning in the branch to which the memoir pertains; and to be accepted for publication only in case the report of this commission is favorable.

5. The commission to be chosen by the officers of the Institution, and the name of the author, as far as practicable, concealed, unless a favorable decision be made.

6. The volumes of the memoirs to be exchanged for the Transactions of literary and scientific societies, and copies to be given to all the colleges and principal libraries, in this country. One part of the remaining copies may be offered for sale; and the other carefully preserved, to form complete sets of the work, to supply the demand from new institutions.

7. An abstract, or popular account, of the contents of these memoirs to be given to the public through the annual report of the Regents to Congress.

II. *By appropriating a part of the income, annually, to special objects of research, under the direction of suitable persons.*

1. The objects and the amount appropriated, to be recommended by counsellors of the Institution.

2. Appropriations in different years to different objects; so that in course of time each branch of knowledge may receive a share.

3. The results obtained from these appropriations to be published, with the memoirs before mentioned, in the volumes of the Smithsonian Contributions to Knowledge.

4. Examples of objects for which appropriations may be made.

(1.) System of extended meteorological observations for solving the problem of American storms.

(2.) Explorations in descriptive natural history, and geological, magnetical, and topographical surveys, to collect materials for the formation of a Physical Atlas of the United States.

(3.) Solution of experimental problems, such as a new determination of the weight of the earth, of the velocity of electricity, and of light; chemical analyses of soils and plants; collection and publication of scientific facts, accumulated in the offices of government.

(4.) Institution of statistical inquiries with reference to physical, moral, and political subjects.

(5.) Historical researches, and accurate surveys of places celebrated in American history.

(6.) Ethnological researches, particularly with reference to the different races of men in North America; also, explorations and accurate surveys of the mounds and other remains of the ancient people of our country.

DETAILS OF THE PLAN FOR DIFFUSING KNOWLEDGE.

I. *By the publication of a series of reports, giving an account of the new discoveries in science, and of the changes made from year to year in all branches of knowledge not strictly professional.*

1. These reports will diffuse a kind of knowledge generally interesting, but which, at present, is inaccessible to the public. Some of the reports may be published annually, others at longer intervals, as the income of the Institution or the changes in the branches of knowledge may indicate.

2. The reports are to be prepared by collaborators eminent in the different branches of knowledge.

3. Each collaborator to be furnished with the journals and publications, domestic and foreign, necessary to the compilation of his report; to be paid a certain sum for his labors, and to be named on the title-page of the report.

4. The reports to be published in separate parts, so that persons interested in a particular branch can procure the parts relating to it without purchasing the whole.

5. These reports may be presented to Congress, for partial distribution, the remaining copies to be given to literary and scientific institutions, and sold to individuals for a moderate price.

The following are some of the subjects which may be embraced in the reports:*

I. PHYSICAL CLASS.

1. Physics, including astronomy, natural philosophy, chemistry, and meteorology.
2. Natural history, including botany, zoology, geology, &c.
3. Agriculture.
4. Application of science to arts.

II. MORAL AND POLITICAL CLASS.

5. Ethnology, including particular history, comparative philology, antiquities, &c.
6. Statistics and political economy.
7. Mental and moral philosophy.
8. A survey of the political events of the world; penal reform, &c.

III. LITERATURE AND THE FINE ARTS.

9. Modern literature.
10. The fine arts, and their application to the useful arts.
11. Bibliography.
12. Obituary notices of distinguished individuals.

II. *By the publication of separate treatises on subjects of general interest.*

1. These treatises may occasionally consist of valuable memoirs translated from foreign languages, or of articles prepared under the

*This part of the plan has been but partially carried out.

direction of the Institution, or procured by offering premiums for the best exposition of a given subject.

2. The treatises should, in all cases, be submitted to a commission of competent judges, previous to their publication.

3. As examples of these treatises, expositions may be obtained of the present state of the several branches of knowledge mentioned in the table of reports.

SECTION II.

Plan of organization, in accordance with the terms of the resolutions of the Board of Regents providing for the two modes of increasing and diffusing knowledge.

1. The act of Congress establishing the Institution contemplated the formation of a library and a museum; and the Board of Regents, including these objects in the plan of organization, resolved to divide the income* into two equal parts.

2. One part to be appropriated to increase and diffuse knowledge by means of publications and researches, agreeably to the scheme before given. The other part to be appropriated to the formation of a library and a collection of objects of nature and of art.

3. These two plans are not incompatible with one another.

4. To carry out the plan before described, a library will be required, consisting, 1st, of a complete collection of the transactions and proceedings of all the learned societies in the world; 2d, of the more important current periodical publications, and other works necessary in preparing the periodical reports.

5. The Institution should make special collections, particularly of objects to illustrate and verify its own publications.

6. Also, a collection of instruments of research in all branches of experimental science.

7. With reference to the collection of books, other than those mentioned above, catalogues of all the different libraries in the United States should be procured, in order that the valuable books first purchased may be such as are not to be found in the United States.

8. Also, catalogues of memoirs, and of books and other materials, should be collected for rendering the Institution a centre of bibliographical knowledge, whence the student may be directed to any work which he may require.

9. It is believed that the collections in natural history will increase by donation as rapidly as the income of the Institution can make provision for their reception, and, therefore, it will seldom be necessary to purchase articles of this kind.

10. Attempts should be made to procure for the gallery of art casts of the most celebrated articles of ancient and modern sculpture.

*The amount of the Smithsonian bequest received into the Treasury of the United States is	$515,169 00
Interest on the same to July 1, 1846, (devoted to the erection of the building)	242,129 00
Annual income from the bequest	30,910 14

11. The arts may be encouraged by providing a room, free of expense, for the exhibition of the objects of the Art-Union and other similar societies.

12. A small appropriation should annually be made for models of antiquities, such as those of the remains of ancient temples, &c.

13. For the present, or until the building is fully completed, besides the Secretary, no permanent assistant will be required, except one, to act as librarian.

14. The Secretary, by the law of Congress, is alone responsible to the Regents. He shall take charge of the building and property, keep a record of proceedings, discharge the duties of librarian and keeper of the museum, and may, with the consent of the Regents, *employ assistants.*

15. The Secretary and his assistants, during the session of Congress, will be required to illustrate new discoveries in science, and to exhibit new objects of art. Distinguished individuals should also be invited to give lectures on subjects of general interest.

This programme, which was at first adopted provisionally, has become the settled policy of the Institution. The only material change is that expressed by the following resolutions, adopted January 15, 1855, viz:

Resolved, That the 7th resolution passed by the Board of Regents, on the 26th of January, 1847, requiring an equal division of the income between the active operations and the museum and library, when the buildings are completed, be, and it is hereby, repealed.

Resolved, That hereafter the annual appropriations shall be apportioned specifically among the different objects and operations of the Institution, in such manner as may, in the judgment of the Regents, be necessary and proper for each, according to its intrinsic importance and a compliance in good faith with the law.

REPORT OF THE SECRETARY.

To the Board of Regents of the Smithsonian Institution:

GENTLEMEN: The duty of presenting to you the annual report of the operations and state of the Smithsonian Institution recurs, on this occasion, under peculiar circumstances. On the 24th of last month, the day before that designated for the annual meeting of the Board, a fire occurred, of which an account is given in the Report of a Special Committee. It destroyed the documents contained in the Secretary's office, and among these was the manuscript of the annual report, which was ready for presentation. The destruction of this involved the necessity of rewriting the whole article, and has delayed its presentation until the present time.

Another circumstance which characterizes our present meeting is that, for the first time in the history of the Institution, not one of those who constituted the original Board of Regents is now in attendance. With the exception of a single member, (Professor Bache,) who is seeking in a foreign country the restoration of his health, an entire change has taken place in the personal composition of the Board. This change has been much more rapid during the last four years, or since the commencement of the war. Within that period, death has repeatedly cast its shadow over the Institution. Indeed, the number of those connected with the establishment who have departed this life since the epoch mentioned exceeds the number in all the years that preceded. The death of Judge Douglas, of Illinois, of Senator Pearce, of Maryland, and that of Dr. Felton, of Cambridge, all prominent members of the Board, were communicated at the last and the preceding meetings; and I have now to add, as having occurred since the last session, the death of General Totten, who was one of the Regents named in the original act of Congress organizing the Institution, and who continued during life to be an active member of the Board, and by repeated election, one of the executive committee; that of Chief Justice Taney, who ever evinced a lively interest in the welfare of the Institution, was one of the original members of the Board, and for a long time held the office of its chairman; and, lastly, that of Judge Dayton, whose decease, in the full enjoyment of

the honors of his high position as the representative of his country at the court of France, we have recently been called to mourn. Though the latter was unable to attend the meetings of the Board, he rendered good service to the Institution in extending its reputation and promoting its correspondence abroad. Besides this mortality among the Regents, there have also occurred in the same period four deaths among the assistants and employés of the establishment, and two among the honorary members, making twelve in number thus removed.

This rapidly recurring mortality has not failed to impress me profoundly with the instability and uncertainty of life, and has led me, in view of the late conflagration and the loss of the counsel of those to whose generous and zealous co-operation I have been so long indebted, to regard with more than usual solicitude the proper discharge of the responsible duties which are intrusted to me as the principal executive officer of the establishment.

Yet, however grieved at the loss occasioned by the fire, and saddened by the departure of those to whom I have just referred, I have not permitted myself for a moment to doubt that I shall continue to find in the present members the same cordial co-operation and liberal support which has characterized the guardians of the Institution for the past twelve years. Whatever may have been the diversity of views previous to that period, no difference of opinion has since been expressed as to the propriety of the general policy which has governed the operations of the establishment, nor has a doubt been intimated as to the value of the results produced or their strict conformity with the intentions of Smithson. This harmony is, perhaps, more worthy of remark, when it is remembered that in the choice of the Regents they have been designedly selected by Congress from each of the prominent political parties of the day. Men of the most conflicting opinions meet here as on a common ground of friendly sympathy, impressed with the feeling that rivalry and prejudice should hold no sway in the presence of interests whose universality and permanency properly withdraw them from the sphere of popular and temporary excitement. Hence my enforcement of the rule excluding from the lecture-room of the Institution topics of a partisan and irritating character has been fully sustained; while, at the same time, the course which has been pursued of rendering the government in its late trials every aid which could be supplied by scientific research has been warmly approved.

As most persons are probably entirely ignorant of the services

really rendered to the government by the Institution, I may here state the fact that a large share of my time—all, indeed, which could be spared from official duties—has been devoted for the last four years to investigations required by the public exigencies. Within this period several hundred reports, requiring many experiments, and pertaining either to proposals purporting to be of high national importance, or relating to the quality of the multifarious articles offered in fulfilment of legal contracts, have been rendered. The opinions advanced in many of these reports not only cost much valuable time, but also involved grave responsibilities. While, on the one hand, the rejection of a proposition would be in contravention to the high importance claimed for it by its author, on the other the approval of it would perhaps incur the risk of the fruitless expenditure of a large amount of public money. It is not necessary, I trust, to say that the labor thus rendered was entirely gratuitous, or that in the judgment pronounced in any case no regard was paid to the interested solicitations or personal influence of the parties concerned; on the contrary, it has in some instances resulted from the examination of materials sold to the government that attempted fraud has been exposed and the baffled speculator received his due reward in condemnation and punishment. These facts, it is thought, will be deemed a sufficient answer to those who have seemed disposed to reproach the Institution with the want of a more popular demonstration, but far less useful or efficient aid in the support of the government.

At the close of 1864 the affairs of the Institution were in a highly prosperous condition. It will be seen by a reference to the report of the executive committee that—

First. The whole amount of money originally derived from the bequest of Smithson is still in the treasury of the United States, bearing interest at six per cent., paid semi-annually, and yielding $30,910.

Second. Seventy-five thousand dollars of an extra fund are in bonds of the State of Indiana, at five per cent. interest, also paid semi-annually, yielding $3,750.

Third. Fifty-three thousand five hundred dollars of the same fund are in bonds of the State of Virginia, twelve thousand in those of Tennessee, and five hundred in those of Georgia, from which nothing has been derived since the commencement of the war.

Fourth. A balance of upwards of $29,000 is now in the hands of the treasurer of the Institution. The only difference in this state-

ment and that of last year is that the balance now in the hands of the treasurer is $2,500 less than before. This difference is mainly due to the increase of prices and the consequent necessity of a greater expenditure in carrying on the ordinary operations.

In view of the great expenditures of the government on account of the war, the Institution did not at first claim, as it might reasonably have done, to have the annual income from the original bequest paid in specie, as all the older funded debts of the United States are paid. But since a large outlay will be required to repair the damages caused by the fire, the necessity could not be avoided of calling the attention of the Secretary of the Treasury to this measure. That this claim is a just one was the unanimous opinion of the Board of Regents, and among them of Chief Justice Chase, and in accordance with the instructions of the Board I have presented this matter to the department. It was referred by the Secretary to his legal adviser, the Solicitor of the Treasury, who has decided that in accordance with the usage of the government the Institution is entitled to receive the interest from the original bequest of Smithson in coin. The premium on this will therefore, in future, increase the balance in the hands of the treasurer.*

It was mentioned in the last report that a part of the original bequest, amounting to £5,015, was left by Mr. Rush in England as the principal to secure an annuity payable to the mother of Smithson's nephew. The annuitant having died, a power of attorney was sent in November, 1862, to Messrs Fladgate, Clark and Finch, (the same firm originally employed by Mr. Rush,) to collect the money. After a considerable delay, arising principally from technical difficulties, the money was obtained and deposited to the order of the Institution, with George Peabody & Co., bankers, London. It was subsequently drawn through the agency of the Secretary of the Treasury, and, in accordance with the law of Congress directing that the money of the Smithsonian bequest should be invested in United States securities, it was expended in the purchase of government bonds, bearing interest at the rate of $7\frac{3}{10}$ per cent. The amount realized in bonds of this denomination, at par, was $54,150. It was at first supposed that this money, or at least the interest upon it, could immediately be applied to the uses of the Institution, but from a critical examination of the enactments of Congress in reference to the Smithsonian fund,

* The premium on the coin received since the presentation of the report, on account of the interest due 1st January, was $7,472 70, which sum added to $29,484 08 gives $36,956 78 as the amount in the hands of the treasurer

it was found that the appropriation of the bequest by the act organizing the establishment in 1846, related only to that part of the bequest which had already been received, and made no provision for the disposition of the residuary legacy which has just become available. It can scarcely be doubted, however, but that Congress intended to appropriate the whole of the bequest to the maintenance of the establishment; still, for this purpose, a special act will be required, and it is desirable that the sum recently received be deposited in the treasury on the same condition with the amount originally obtained, that the interest alone shall be subject to expenditure. In this connexion it is proper to remark that Mr. Peabody, who received the deposit of the fund, so far from claiming the usual commission, allowed four per cent. on the money while it remained in his hands.

It will be seen from what follows in this report that all parts of the programme have been prosecuted during the past year with as much energy as the means at our disposal would permit, and that although, in some particulars, not as much has been accomplished as in previous years, the inequality will, it is hoped, be attributed, as it is properly referable, to the difficulties under which the Institution, in common with the whole country, has been laboring.

Publications.—The whole number of pages issued during the year amount to 872 quarto and 1,657 octavo.

The thirteenth volume of the Contributions has been distributed to public libraries, and the fourteenth is nearly completed, and will be published in the course of a few months. It will consist of the following papers:

1. The third and fourth series or concluding parts of the discussion of the magnetic and meteorological observations, made at the Girard College observatory, Philadelphia, by Professor A. D. Bache, Superintendent of the United States Coast Survey.

2. On the construction of a silvered glass telescope, 15½ inches in aperture, and its use in celestial photography, by Dr. Henry Draper, of the University of New York.

3. A memoir on the palæontology of the Upper Missouri, by F. B. Meek and F. V. Hayden.—Part 1.

4. A memoir on the cretaceous reptiles of the United States, by Dr. Joseph Leidy, of the University of Pennsylvania.

It was intended that Dr. Dean's paper on the medulla oblongata, described in the last report, and partially distributed in separate num-

bers during the year, should form a part of this volume ; unfortunately, however, the plates intended for its illustration were destroyed in the fire, and its place in the volume has been supplied by the fourth article, which has since been presented for publication.

In the reports for the last three years an account has been given of a series of papers containing the deductions from the magnetic observations at Girard College, Philadelphia, by Professor A. D. Bache, Superintendent United States Coast Survey. The whole of this series of papers was divided into four sections, each containing three parts. The object of the whole series is to present the results deduced from the changes observed in the direction and intensity of the magnetic force of the earth as apparently affected by the position of the sun and moon relative to the earth and to each other.

The first section related to the disturbances in the line of the declination, or of the fitful variation, as it is called, of the magnetic needle, and to the regular variations of the declination.

The second section related to the variation in the intensity of the magnetic force of the earth, estimated in a horizontal direction.

The third section related to the same force as estimated in a vertical direction.

The fourth section relates to the perturbations or fitful changes in the direction and intensity of the total magnetic force of the earth as estimated in the direction of the dipping needle.

The first three of these sections have been described in previous reports, and it now only remains to give an account of the fourth and last.

The data for the deductions given in this section are the quantities observed in the variations of the horizontal and vertical components of the magnetic force, expressed in minute scale divisions corrected for progressive changes in the magnetism of the bars and for changes due to temperature. The object of the investigation was to determine the law of the great disturbances to which the total intensity and direction of the magnetic force of the earth is subjected. It is well known that the intensity and direction of the magnetic force of the earth do not remain the same from hour to hour, but are subject to regular fluctuations connected with the day and the season, and also to larger perturbations, which have until lately been considered fitful, and have therefore received the name of magnetic storms. The special object of investigation of the first part of the fourth series is to ascertain the average character of the large disturbances, and to

deduce, if possible, the law by which they are governed. This is the most intricate part of the whole series, and is the final object to which the preceding investigations were preparatory. From a careful study of all the observations on the dip, it was found that 1,446 might be considered as giving abnormal values, and of the total force of the earth's magnetism 1,470 indicated abnormal changes, which amounted to about one-fifteenth part of the whole number of observations. These abnormal disturbances were analyzed in relation to their frequency during the hours of the day, the month of the year, and successive years; they were also studied as to their tendency to exhibit an increase or diminution in their variations and the times of the greatest and least action in relation to the periods above mentioned. This part ends with a table of the relative magnitude of the disturbances and a comparison of those of Toronto and Philadelphia, from which it appears that in some cases there is an agreement in the character of the simultaneous changes in the two places, and at others not.

The second part of the fourth section treats of the solar diurnal and annual inequality of the dip and total force, that is, of the changes due to the sun which take place in the dip and total force from hour to hour and from month to month. In this investigation all the greater perturbations are omitted and the laws of the simpler or more normal changes are sought. The diurnal changes in the dip are shown analytically and graphically for each month and for the whole year. The general character of the curve exhibits a maximum at about 11 a. m. and a minimum at about 5 a. m., with a range of one minute and two-tenths—a quantity too minute to be recognized by the ordinary dip circle, and which can only be observed by the differential reflecting instrument. In summer the epochs occur earlier, with a range of a minute and a half, while in winter they occur later, with a range of only one minute. There is also a secondary fluctuation of small magnitude. The diurnal deviation of the dip is greatest about the time of the equinoxes, and of these maxima that of the winter is the least. The diurnal changes in the total force as deduced from the average of the year are represented by a single crested curve, but from the average of the observations in winter alone this assumes the form of a double curve. The principal maximum as deduced from the whole year coincides with the hour of 2 p. m., and in winter occurs about an hour and a half earlier. The principal minimum coincides with 10 p. m., and occurs in winter about two hours earlier. This part ends with an attempt to deduce from the data the annual changes in dip and intensity. The result, how-

ever, is not entirely satisfactory on account of the large disturbances due to variations of temperature, changes of the magnetism and adjustment of the instrument.

The third part of the fourth section, or the twelfth part of the entire series, contains the result of the observations made with a portable dip circle constructed by Robinson, of London, the same which had been previously used by Professor Bache in his magnetic observations in Pennsylvania and adjacent States, and also in Europe. The observations were made weekly during a period of nearly two and a half years; the monthly and annual mean observations of the dip were tabulated and were found to indicate an annual decrease of one minute and two-tenths in this element. The same paper contains a collection of observations on the dip at different points in Philadelphia by different observers, from which a similar change in the dip has been deduced. The least dip occurred in January, 1840, and increased for several years after that date. It is probable, however, from some subsequent investigations by Mr. Schott of observations at other places, that the minimum obtained at Philadelphia, above mentioned, was of a secondary character, and that a still smaller dip will hereafter be observed. But this point will be cleared up in a few years by observations now in process of collection. The discussion of this part, and indeed the whole of the series, ends with a table of magnetic constants for Girard College, namely: of the declination, or variation, as it is sometimes called; of the dip; of the horizontal, vertical, and total force, all expressed in absolute measures, for five different epochs and for one mean epoch, that for January, 1843, for which the declination is 3° 32′ W., the dip 71° 59′ N., the horizontal force 4.173, the vertical force 12.83, and the total force 13.49, in units of one foot, one grain, and one second of mean time.

From all the investigations on this subject up to the present time we may infer, first, that the earth is a great magnet, having a natural, and in one sense a permanent, polarity; second, that this polarity is disturbed in intensity and direction by the varying effect of the heat of the sun; third, that the magnetism of the earth is affected by that of the sun and moon; and fourth, it is probable that magnetic polarity is common to all the bodies of the solar system.

The second paper in the thirteenth volume of the Contributions—that on the silvered glass telescope—is fully described in an article at the end of this report, copied from the "Intellectual Observer," of London.

The third paper in the volume, on the palæontology of the Upper Missouri, was described in the last report. It occupies 158 pages, and is illustrated by five plates of figures engraved on stone.

A full account of the fourth paper, on the cretaceous reptiles of the United States, as given by the author, is also appended at the close of this report.

Miscellaneous Collections.—On account of the continued increase in the price of printing and paper, and the unexpected length to which some of the works were tending, I thought it advisable to suspend, for the present, the general publication of this series. Of the list of works it comprised, as given in previous reports, the only ones published since the last session of the Board are the second part of Binney's Bibliography of American Conchology, Meek's Check Lists of Fossils, and the supplement to Loew's Diptera.

The first of these contains an account of the writings of foreign naturalists relative to American conchology, and also additions and corrections of the first volume, with a copious index of authors and names of species. It forms an octavo volume of 300 pages.

The second work consists of check lists of all the species of cretaceous, jurassic, and miocene invertebrate fossils of North America which had been described up to the end of 1863. These constitute an important aid in the labor of cataloguing and labelling collections. The manuscript of another number of the same series, prepared by Mr. Conrad, of Philadelphia, has been received. It gives a list of the eocene invertebrate fossils, and, as the work is much wanted to assist in the distribution of specimens, it will be put to press immediately.

The other article of the Miscellaneous Collections published during the past year is the supplement and completion of the second part of the monograph of the Diptera of North America, by H. Loew. A general account of the work on the diptera (comprising flies, musquitoes, &c.) is given in the report for 1861. This order of insects has perhaps a wider distribution than any other known, and, from the variety and the minuteness of the specimens, is difficult of study and classification. Before attempting to give a monograph of the whole order, it was thought proper to print a catalogue of all the genera which had been described, and this work (prepared by Baron Osten Sacken) was published in 1858. The preparation of the monograph was intrusted to Dr. H. Loew, of Meseritz, Prussia, one of the most eminent naturalists in this line now living.

In the first part of this work is an essay on the terminology of

diptera, a sketch of the systematic arrangement of the order, with the genera found in North America. It occupies 221 pages, and is illustrated by two plates. The second part is occupied with a monograph of the American Dolichopodidæ. For a large portion of the materials on which both parts of this work are based the Institution is indebted to the liberal assistance of Baron Osten Sacken, though some interesting species were communicated to Mr. Loew by Mr. Le Baron, of Illinois, and by Professor Macklin, of Helsingfors, collected by Mr. Sahlberg. The types of a collection were also lent to him by the directors of the *Hof Naturalien Kabinet*, of Vienna.

Although the materials placed at the disposal of the author were large, they did not reach the extent desired for the preparation of a complete monograph. The hope is, therefore, expressed that additional collections will be made to complete the work, and for this purpose the request is earnestly urged on all North American collectors who take an interest in this order of insects, to favor the enterprise by sending specimens to the Institution, which may be transmitted to Dr. Loew.

The fauna of North American Dolichopodidæ far exceeds the European in the variety of forms and in the number of species. A striking circumstance connected with this class of insects as found in North America is their remarkable analogy to the remains of the fossil fauna of the same family preserved in amber. In both there is the same abundance of species of a particular genus, difficult to distinguish on account of their close resemblance. It would appear from this, that if there is a gradual variation of species under varying conditions of existence, this variation has been less in regard to American insects of this class than in those of Europe. It is important in the progress of science not only to trace the limits of different faunas, but to compare those of a similar class in different countries. At present, however, this cannot be done with any degree of precision, except in the case of the American and European insect fauna. In this case it is distinctly perceived that the two approach each other in the species of several genera, while in others the species are identical, and again those which are identical in both are very unequally represented in the two countries. Of the species common to Europe and North America, it is not improbable that some of them should have been accidentally imported in ships from the former.

The second part, including the supplement, consists of 371 pages, and is illustrated by five plates.

Two other works of the same series were completed, and would have been immediately published had the manuscripts not been destroyed by the fire. The first of these was a monograph of the Myriapoda, by Dr. H. C. Wood, and the other a monograph on the Limnobina, by Baron Osten Sacken.

Reports.—The annual reports to Congress are printed at the expense of the government as public documents, with the exception of the wood-cuts, which are furnished by the Institution; and it is gratifying to be able to state that for a number of years there has not been a dissenting vote in Congress on the adoption of the order to print the usual number of ten thousand extra copies of this work. The manuscript of the report for 1863 was unfortunately mislaid at the Capitol, and the public printer was therefore obliged to delay the publication on account of other more pressing demands of the departments of the government. It is much to be regretted that at the recent fire at the Institution all the copies of the reports on hand for general distribution to individuals were destroyed, so that at present it will be impossible to supply the many applications which are made for copies of the back volumes of the series. The reports for 1861 and 1862 were stereotyped, and when the cost of press-work and paper is reduced to its normal state, a new edition of these may be struck off and disposed of at the mere price of production.

The report for 1863 contains in the appendix a course of lectures on the principles of linguistic science, by Professor W. D. Whitney, of Yale College; a eulogy of Beautemps-Beaupre, translated by C. A. Alexander, esq., a continuation of the series of memoirs of distinguished members of the French Academy of Sciences; an account of the origin and history of the Royal Society of London, prepared by the same; an exposition of the modern theory of chemical types, by Dr. Charles M. Wetherill; an original article on the method of preserving Lepidoptera, with illustrations, by Titian R. Peale, esq.; an account of a remarkable accumulation of bats at the residence, in Maryland, of M. Figaniere, Portuguese minister; a number of articles on ethnology, giving an account of ancient remains in various parts of America and Europe. There are also a number of translations made expressly for the Institution, viz: researches on the phenomena which accompanied the propagation of electricity in highly rarefied elastic fluids, by Professor de la Rive; report on the proceedings of the Society of Physics and Natural History of Geneva, by Professor Marcet; the commencement of Plateau's researches on the figures of

equilibrium of a liquid mass withdrawn from the action of gravity; an account of the history of discovery relative to magnetism; recent researches relative to the nebulæ, by Professor Gautier; an article from the annals of the Observatory at Madrid, by Miguel Merino, on the investigations made to determine the form and volume of the earth; Arago's account of aeronautic voyages performed with a view to the advancement of science, to which is added from an English publication Mr. Glaisher's account of his recent ascensions in England; the first part of an interesting and valuable account of the aboriginal inhabitants of the Californian peninsula, by Baegert, a Jesuit missionary who lived there seventeen years during the second half of the last century; and an article from a German scientific periodical on purple and azure dyeing in ancient and modern times. At the end of the volume a few of the more important tables of weights and measures, especially needed for reference in some of the preceding articles, have been added.

Ethnology.—The publications of the Institution relative to ethnology during the past year are those given in the appendix to the last report, the most important of which is a translation by Professor Rau, of an account of the aboriginal inhabitants of the Californian peninsula, by Baegert, a German Jesuit missionary. The book from which this translation was made was published in Germany in 1773, and is now very scarce and almost unknown in this country. It will be considered, we doubt not, at this time, an interesting contribution to the ethnology as well as the early history of a part of the world which has of late years occupied so much of the public attention. Mr. Rau has not given a translation in the strict sense of the word, but a reproduction of the work only so far as it relates to ethnological matters, his object being to rescue from oblivion facts relating to the history of a portion of the American race. The second part of this work will be published in the appendix to the present report.

There is a growing taste for the study of ethnology in this country, and consequently a desire to form collections illustrating the condition of the American aborigines in different parts of the continent. In order to encourage this tendency, and to bring together for critical study and comparison the scattered specimens which exist in this country, the Institution has requested, either as a gift or a loan, specimens of the arts and other remains found in mounds, excavations, or on the surface of the ground; and with the assistance of Professor Matile, formerly of the University of Neufchatel, commenced in 1863

the preparation of a series of moulds from which casts are made for distribution and exchange. In carrying on this work we have been favored with a large collection of specimens of Mexican art, principally images and masks, by the American Philosophical Society, of Philadelphia, from which moulds have been taken. The prosecution of this work has been temporarily suspended, but will be resumed as soon as facilities and means for its prosecution can be provided. In this connexion we would renew the request which we have made in previous reports, that descriptions of all mounds or aboriginal earthworks which may be discovered may be sent to the Institution for the purpose of furnishing the materials for a work at some future time on the distribution and migration of the ancient inhabitants of this continent. In order to preserve and render generally accessible the information which may be obtained in this way, it will be published in the appendix to the next succeeding annual report after its reception.

Meteorology.—It has been mentioned in previous reports that the second volume of the results of meteorological observations made under the direction of the Smithsonian Institution and the Patent Office, from the year 1854 to 1859, was in press, and that its completion was delayed by the unusual amount of printing required by the necessities of the public service to be executed at the Government Printing Office. It was thought best, therefore, to issue the portion already printed, without waiting longer for the other material which it had been intended to embrace in the volume. This portion, forming a quarto volume of more than five hundred pages, was consequently bound and distributed during the past year. It is divided into two parts, each occupying about half the volume. The first relates to the periodical phenomena of plants and animals from 1851 to 1859, inclusive, embracing observations upon the foliation of eighty-seven species, the blossoming of ninety-two, the ripening of fruit of ten, and the defoliation of eighteen species of plants, and upon the first appearance of sixteen species of birds, one of reptiles, three of fishes, and two of insects. These results have a direct application to meteorological science, by indicating the progress of the seasons in different localities, and their relative variability in different years. To these have been added several tables of the opening and closing of lakes, rivers, canals, and harbors, collected from various sources, and tending to illustrate the same leading features of climate as the records of organic phenomena. The materials were furnished chiefly by the regular Smithsonian observers, and were arranged and prepared for publication by Dr. Franklin B. Hough, of Albany, N. Y.

The latter half of the volume is occupied with materials for the critical study of three storms in 1859, one of which occurred in March, and the other two in September, collected from the records of the Institution, and prepared for publication by Professor J. H. Coffin, of Lafayette College, Easton, Pennsylvania. One of the important objects aimed at in establishing the meteorological observations of the Smithsonian Institution was the collection of data for the critical examination of the development and progress of the extended commotions of the atmosphere which occur during the autumn, winter, and spring, over the middle or temperate portions of North America. It is well known that two hypotheses as to the direction and progress of the wind in these storms have been advocated with an exhibition of feeling unusual in the discussion of a problem of a purely scientific character, and which, with sufficient available data, is readily susceptible of a definite solution. According to one hypothesis the motion of the air in these storms is gyratory; according to the other it is in right lines toward a central point, or toward an irregular elongated middle space. It is hoped that the data here given will be considered of importance in settling, at least approximately, these questions as to the general phenomena of American storms.

These two quarto volumes of meteorological results for the six years 1854 to 1859 inclusive, embracing nearly two thousand pages, together with a volume covering very nearly the same period of time published by the War Department, probably form an unsurpassed body of materials for the investigation of meteorological phenomena over so wide an extent of country. The tables of the War Department embrace nearly two hundred quarto pages of reductions for five years, 1855 to 1859, inclusive, and form an appendix to the "statistical report on the sickness and mortality in the army of the United States," published in 1860, compiled by Assistant Surgeon R. H. Coolidge, under the direction of Dr. Lawson, Surgeon General United States army. The original records, both in the Smithsonian Institution and War Department, from which the results contained in these three volumes were deduced, are open to the examination of persons who wish to make investigations more minute, or of a more extended nature than can be embraced in general tables.

It is regretted that we have not the means at present of continuing the reduction of all the records as received from the observers, and of publishing the results. This want, however, is supplied to a limited extent by the publication of the reductions of temperature and

rain in the monthly report on the state of the crops and the weather, issued by the Agricultural Department, between which and this Institution the relations mentioned in the last report have been maintained through the past year. To save postage, the blank forms have been sent out and the registers returned through the frank of the office of the Commissioner of Agriculture. The monthly bulletin above referred to, which is printed at the expense of the same department, continues to be received by the public with much favor; and, by means of its extensive distribution, presents the meteorological tables to a much larger circle of readers than is comprised in the list of our observers, awakening, to a corresponding extent, an interest in the subject of meteorology. This branch of science is receiving increased attention from year to year, and a larger number of individuals are devoting time and talent to efforts for unfolding the laws which control the formation and movement of vapor, winds, and change of temperature in all parts of the world. Meteorology has ceased to be a mere record of isolated facts. The special characteristic of modern efforts in this line consists in extended cooperation, and in determining the simultaneous condition of the atmosphere over extended regions of country. It is only by this means that the laws which govern the occurrence, motion, direction, and propagation of the disturbances of the atmosphere can be ascertained. By comparisons of this kind isolated observations of otherwise little value become important, and afford an ample field in the cultivation of which any person who will take the trouble to record the direction of the wind, the beginning and ending of rain, snow, hail, the time of blossoming of trees, appearance of birds, insects, &c., may render valuable service.

The daily record of meteorological observations telegraphed to the Imperial Observatory at Paris, and published in a lithographed sheet, continues to increase in interest and importance under the active and enlightened superintendence of M. Le Verrier, director of the observatory. From being the medium simply for the circulation of telegraphic notices of the weather, it has become, in addition, a repository of valuable meteorological summaries, communications, criticisms, and announcements. The outline chart of Europe, with the curves of equal barometric pressure and direction of the wind at the different stations on the day of publication, and also a table of the estimated weather for the following day, continue to be inserted in every number. The title of the publication is now "International Bulletin of the Imperial Observatory of Paris." It occupies more than

twelve hundred folio pages yearly, at a subscription price of thirty-six francs.

The Institution has also received a similar meteorological bulletin from the Royal Observatory at Palermo. In the first number of this, a plan is proposed for distributing simultaneous meteorological observations similar to that which was adopted previous to the war by the Smithsonian Institution, viz: that of furnishing the most important telegraphic stations with meteorological instruments, and instructing the principal telegraphist, or one of his assistants, in the process of making observations. A thoroughly organized system of this kind over the whole United States, with a series of directions for predicting the weather at a given place from a knowledge of the condition of the atmosphere at distant points, would be of vast importance to the maritime and agricultural interests, particularly along the Atlantic sea-board. It is hoped that as soon as order is restored and peace fully re-established throughout the southern portion of the United States, the system will be revived under still more favorable auspices.

An important addition to the means at the command of the Institution for this purpose has been furnished by the liberal action of the North American Telegraphic Association, in giving the free use of all its lines for the scientific objects of the Institution. The association embraces the Western Union, the American, the Montreal, the Southwestern, and the Illinois and Mississippi Telegraph Companies, covering the entire United States and Canada, including the overland line to San Francisco, which, by its charter, is required to transmit without charge scientific despatches for the Institution. The telegraph companies on the Pacific coast have also liberally granted the same privileges.

I am happy to state in this connexion that efforts have been made to revive and complete the meteorological observations which were collected by the Naval or National Observatory. The records from the log-books of the commercial and naval marine collected under the direction of the former superintendent, though imperfectly, and in many cases erroneously interpreted, were valuable contributions to the materials from which the true theory of the general motions of the atmosphere are to be deduced.

The lake system of meteorology is still kept up under the new superintendent, Col. Raynolds, though the Institution has not received the copies of the registers for the past year.

The State Department has furnished the Institution with several meteorological contributions forwarded to it by consuls in foreign

countries. Among them are observations made at Constantinople for the year ending September, 1863; daily telegraphic reports of the weather in Europe, communicated to the Central Physical Observatory at St. Petersburg, Russia, for the year ending September, 1864, translated and compiled by Mr. Edwin Phelps, United States consul; meteorological review for the year 1864, from observations at the Leprosy hospital of Lungeguard in the city of Bergen, Norway, reduced by O. E. Dreutzer, United States consul; monthly tables for a part of the year 1864, from the consul at Turk's Island, West Indies. If all the American consuls in foreign countries would collect and send to the State Department local publications containing meteorological tables, many valuable additions might be furnished.

The Navy Department, as heretofore, has transmitted to the Institution monthly reports kept at the naval hospitals at Chelsea, New York, and Philadelphia.

A circular and a chart of stars prepared by the Connecticut Academy of Arts and Sciences was published by the Smithsonian Institution, and distributed to its observers for the purpose of obtaining records of the meteors that might appear on the night of November 13–14, 1864, but the general cloudiness of the night prevented the attainment of any valuable results.

The three rooms in which the meteorological records were kept were destroyed by the fire on the 24th of January, 1865. Owing to the great rapidity with which the fire progressed much valuable material was lost, but fortunately the larger portion of the contents of the rooms were saved. Among the articles lost were the principal instruments used at the Institution for meteorological observations, including the self-registering apparatus for recording the direction and velocity of the wind, constructed by Dr. Smallwood, of Montreal, and partially described in the Smithsonian reports for 1856 and 1860. It had been in operation since 1858. All the records kept by it were lost. As soon as a minute investigation can be made as to the missing sheets of the general records, a list of deficiencies will be published, and it is hoped that a portion at least of these may be restored by copies of the duplicates retained by the observers.

Laboratory.—During the past year the laboratory has been in charge of Dr. Charles M. Wetherill. The experiments mentioned in the last report on materials for light-house illumination have been continued, and a series of examinations has been made of different substances submitted for that purpose by the government. The most

extended series of experiments, however, has been that which relates to the condition of the air, and the mode of ventilation of the United States Capitol. This subject was referred by Mr. Thomas U. Walter, the architect of the Capitol, to the Secretary of this Institution. The plan of the investigation having been determined, the experiments have principally been made by Dr. Wetherill. The result of this investigation, it is believed, will not only throw additional light on the points for which it was instituted, but also form an interesting addition to the subject of ventilation. The work in the laboratory, also by Dr. Wetherill, comprised various researches upon subjects of chemical science. Of these, three, viz.: "On the nature of the so-called ammonium amalgam;" "On the crystallization of sulphur;" and "On the crystalline nature of glass," will be published shortly in one of the scientific journals of the country. The means for carrying on physical research at the Institution have been materially diminished, on account of the destruction by fire of the very valuable collection of physical apparatus. Fortunately the conflagration did not extend to the laboratory, and consequently the chemical apparatus was preserved.

Collections of specimens of natural history, &c.—The work of making collections of specimens of natural history has been prosecuted as in previous years. A very large collection of mammals, birds, eggs, &c., made in the northern part of British America in 1863, principally by the officers of the Hudson's Bay Company, has arrived at Fort Garry, and is expected soon to be received in Washington. Collections have also been received from Labrador, Puget's sound, and from various parts of the United States, Central America, Mexico, and the West Indies, a detailed account of which is given in the annexed report of Professor Baird. Advantage has been taken of every exploring expedition which has been sent out by government, and in many cases of the assistance offered by officers of the army, particularly of the medical department, for adding new materials or duplicate specimens to the collections. The great object, as has been frequently stated before, of this work, is to obtain the materials for an extended knowledge of the natural history of this continent, and to furnish illustrations of type specimens to museums, colleges, and other educational establishments.

The whole number of specimens catalogued during the last twelve years is upwards of 100,000, and including duplicates, the whole number collected will amount to five times that amount.

The distribution of duplicates has been continued as rapidly as the identification and labelling could be accomplished. In this distribution regard has been had to the relative geographical positions of the establishments to which the first sets of specimens have been sent as well as to their importance as influential centres of higher education. According to the statement of Professor Baird, it will be seen that already upwards of 16,000 specimens have been distributed during the year, and efforts will be made during the season to increase this number. The importance of this branch of operations depends more upon what the Institution is enabled to distribute than on what it accumulates for permanent preservation.

Museum.—The type specimens of the museum have been gradually increased during the past year, not only from the collections made by the Institution, but also from donations received from abroad, particularly as regards rare birds, eggs, fossils, and animals. The European specimens of ornithology were requested for the purpose of enabling Professor Baird by comparison to prosecute his work on American birds.

Previous to the fire the large room partly occupied by the Stanley collection of Indian portraits had been fitted up with about two hundred feet of cases around the walls, to receive the ethnological specimens in possession of the Institution. While engaged in re-arranging the pictures above these cases, the workmen, with a view to their own comfort, unfortunately placed the pipe of a stove in a ventilating flue which opened under the roof, and thus caused the conflagration which destroyed the upper part of the main building. Fortunately none of the ethnological articles had been placed in this room, and consequently these specimens, with those of the museum and of the general collections, have been preserved.

Exchanges.—The system of international literary and scientific exchanges has been continued during the past year with unabated energy, and on the part of the Institution exclusively, several hundred sets of its publications, each embracing 1,782 pages, have been sent to foreign institutions.

According to the tabular statement given by Professor Baird it appears that, during the year 1864, there have been despatched to foreign countries 1,011 packages, each containing a number of articles, enclosed in sixty-three boxes, measuring 546 cubic feet and weighing 20,500 pounds. The number of packages received in return for societies and individuals in this country was 2,482 (nearly twice as many as in 1863) exclusive of those for the Smithsonian library.

original research in the various branches of knowledge, and had doubtless a proper appreciation of the good which might be effected by founding an institution especially adapted to advance this object. He accordingly intrusted his property to the United States to found an establishment "*under the name of the Smithsonian Institution for the increase and diffusion of knowledge among men.*" He evidently did not intend by these precise terms to found a library or a mere museum for the diffusion of popular information to a limited community, but a cosmopolitan establishment to *increase* the *sum* of human knowledge and to diffuse this to every part of the civilized world. No other interpretation of the will is either in accordance with the terms employed or with the character and habits of the founder. The *increase* of human knowledge, by which we must understand additions to its sum, would be of but little value without its diffusion, and to limit the latter to one city, or even to one country, would be an invidious restriction of the term *men*. These views, so evident to minds especially devoted to science, were not at once apparent to those whose studies and pursuits had been chiefly confined to literature or public affairs. The first scheme which was presented in regard to the character of the future institution proposed that it should assume the form of a university, but this idea was shown to be erroneous by the Hon. J. Q. Adams, who pointed out the fact that the object of a university was not to increase knowledge, but to diffuse that which already exists. The next proposition, which had many advocates, was that of a large library or museum; but these objects are in a measure local in their influence and tend, like the former, to promote rather the diffusion than the increase of knowledge.

From this diversity of opinion as to the character of the proposed Institution, or from whatever other causes, the bequest was suffered to remain inoperative for eight years. It was not until 1846 that Congress passed the act of organization under which the Institution has since continued in operation.

This act directs that provision be made for a library, museum, and gallery of art, in a suitable building of plain and durable materials, and after these and some other general indications of the views of the legislature, leaves it discretionary with the Board of Regents to adopt such further measures for promoting the common purpose as might seem, in their judgment, best to comport with the terms of the donation.

I may be permitted to state, without giving undue prominence to my own part in the organization, that immediately after the passage

of this act I was requested by one of the Regents to prepare a sketch of such an institution as I deemed that of Smithson ought to be, with reference at once to the requirements of Congress, and the brief, though comprehensive, phrases of the will. After devoting careful attention to the expressions of the bequest, and being acquainted with the character of the founder, I could not entertain the slightest doubt that it was the intention of the latter to establish a cosmopolitan institution, which should be alike a monument of his own fervent love of science, an efficient instrumentality for promoting original researches and rendering a knowledge of their results accessible to inquiring minds in every part and age of the world. I accordingly advised the adoption of the plan set forth in the first section of the programme presented to the board in my report for 1847,* a plan which is principally designed to increase knowledge by instituting researches and assisting in various ways men of talents and acquirements to make original investigations in all departments of scientific inquiry, as well as to diffuse the knowledge thus obtained by presenting, free of cost, to all the principal libraries and public institutions of the world copies of a series of volumes containing the results of the investigations instituted.

Previous to the presentation of these views, one of the Regents had reported in favor of making immediate provision for a library, a museum, a gallery of art, and other local objects, in connexion with a system of lectures to be delivered in different parts of the country; while another Regent had presented an eloquent appeal in favor of a great library composed of books in all languages and on all subjects.

In reviewing these and other plans of organization which had been previously advocated, it will scarcely be denied by an unprejudiced mind that, for the most part, they were such as to exert a merely local influence, and which, if they embraced means for the diffusion of popular knowldge, neglected the first and essential condition of the bequest, viz.: the *increase* of knowledge—in other words, the advancement of science or the discovery and promulgation of new truths. On the other hand, the plan of organization presented in the first section of the report for 1847 is that of a living, active, progressive system, limited in its operations only by the amount of the income; calculated to affect the condition of man wherever literature and science are cultivated, while it tends in this country to give an impulse to original thought, which, amidst the strife of politics and the inordinate pursuit of wealth, is, of all things, most desirable.

* See programme of organization, page 8 of this report.

These views, which have commanded the approval of unprejudiced and reflecting persons generally, and especially of men of science, to which class Smithson belonged, were fully shared from the first by Professor Bache, General Totten, Gideon Hawley, esq., and in whole or in part by other members of the board, and I was elected the secretary or principal executive officer, to develop and carry into practice, as I supposed, under the direction of the board, the plan I had suggested.

The appointment was accepted with much and not causeless solicitude as to the result. I soon found that although a number of the members of the board were in favor of the promotion of original researches, or of what has since, by way of discrimination, been called the active operations, neither a majority of the Regents nor perhaps the community in general was prepared to favor a plan of organization which should exclude the material representation of the Institution in the form of an extensive architectural structure calculated to arrest the eye and embellish the national capital.

It was in vain to urge the fact that a large and expensive building was not only unnecessary to the realization of the purpose of Smithson, but that it would tend to defeat that object by absorbing the income, controlling the future policy of the Institution, and confining its influence principally to a single locality; that it was not the estimated first cost of the edifice which should alone be considered, but also the expense of keeping it in repair and the maintenance of the corps of assistants and employés which would be required in an establishment of this kind; that the increase of the collections of a miscellaneous library and public museum would, in time, require additional space; and that, finally, all the revenue of the bequest would be absorbed in a statical establishment, or in attempting to do that which can only be properly accomplished, as in other countries, by means of the government. Unfortunately the building committee had settled upon a design for the building in the Lombard style, and Congress had presented to the Institution the museum of the exploring expedition, then at the Patent Office, and directed that provision should be made on a liberal scale for its accommodation, neglecting, at the same time, to fill the blank in the act of organization, by which the cost of the building was to have been limited. It was this provision of the law which furnished a fulcrum for the influence exerted by the citizens of Washington, and persons pecuniarily interested, directly or indirectly, in contracts or otherwise, in favor of the erection of the present structure. Thus

re-enforced, the fascination of its architectural display as presented on paper proved too strong to be resisted.

The adoption of this extensive and costly building was considered so inauspicious a beginning that I had resolved to resign the office of director, and make no further attempt to introduce the plan with a view to the success of which I had accepted the position, when a temporary compromise was proposed by which the several plans might be brought to the test of experience, and an opportunity apparently given for any modifications which might be found advisable.

In order to meet the large expenditure on the building, to provide for the support of the establishment necessarily connected with it, and to leave the greater part of the interest of the original bequest free to be applied to its more legitimate objects, it was resolved to create an extra fund, while gradually developing the plans of organization, and for this purpose the following course was adopted:

1. The building to be erected in parts, and its different portions gradually brought into requisition, its completion being thus delayed for a number of years.

2. The sum appropriated to the building, furniture, and grounds, viz., $250,000, being mainly the interest which had accrued previous to the organization, to be invested in United States treasury stock, bearing interest.

3. The plan of organization to be gradually developed, and, instead of expending upon it from the first the whole interest of the original bequest, a part of this to be also invested in treasury bonds.

4. The remainder of the income to be divided between the active operations on the one hand and the library and museum on the other. The latter to be restricted principally to scientific books and to type specimens.

This compromise was adopted, and has been so successfully and steadily carried out financially, that at the commencement of the war, after paying for the building, accumulating a very valuable library, establishing and supporting a large museum, and carrying on all the active operations of the establishment, an extra fund had been created amounting to $140,000. In order to secure this from the contingencies of any future expenditure on buildings or loss from hazardous investment, a petition was preferred to Congress to take it from the care of the Regents and deposit it with the original principal in the treasury of the United States, subject to the same restriction, viz., that the interest alone could be expended. This petition not having been acted upon, the Regents

deemed it expedient to invest the money in such State stocks as were then considered most eligible, and accordingly there were invested in—

Indiana	5 per cent. stock	....	$75,000
Virginia	6 " " "	....	53,500
Tennessee	6 " " "	....	12,000
Georgia	6 " " "	....	500
Washington	6 " " "	....	100
	Amounting in all to	....	141,100

This scheme has afforded an ample opportunity to compare the relative advantages of the two principal plans of organization and to verify the predictions which were originally made in regard to the building. Though but a portion of the income has been devoted to the active operations, they have produced results in the way of increasing and diffusing knowledge abundantly sufficient to justify the anticipations which were entertained in regard to them, and to convince the most skeptical of their primary importance. As to the building, it is now abundantly proved that a structure of one-fifth of the cost would have been sufficient for the wants of the Institution, and that two grave errors were committed in the adoption of the present one: first, the plan was but little adapted to the uses to which the edifice was to be applied; second, the style of architecture required a far greater expenditure than the amount to which the cost of the building was limited. For the purpose of architectural effect the interior was very inconveniently divided; the buttresses, turrets, and towers, while they add very little to the accommodation of the building, greatly increased the cost. To have constructed the building in a substantial and durable manner, in strict conformity with the Lombard style of architecture which was adopted, would have required an expenditure of at least double the amount of the sum appropriated for the purpose. It was, therefore, necessary, in order that the exterior might be constructed in freestone, that the interior should be finished in wood and stucco, and that thus recourse should be had to the presentation of a falsehood to the eye in the very inauguration of an enterprise for the advancement of truth. The two wings and the two connecting ranges were completed in this manner. The main building, which is 200 feet long and 50 wide, embellished with six towers, was also in process of completion, the framing of the interior having been finished, when the underpinning gave way and the whole of the woodwork fell to the ground. After the occurrence of this accident a commission of architects, appointed

to examine the building, reported that the exterior walls were well built, both in regard to construction and materials, but that the plan of finishing the interior in wood and stucco was improper for an edifice intended to contain valuable articles; it was therefore recommended that fire-proof materials should be employed for the portions of the work which remained to be constructed. In conformity with this recommendation the interior of the main building was completed in iron, stone, and brick, with the exception of the roof, which, being covered with slate and not supposed to be exposed to danger from fire, was suffered to remain. It was this change, in the mode of constructing a portion of the edifice, which, during the late fire, saved the contents of the whole from destruction. It, however, increased the cost of the building to upwards of $300,000, leaving the remaining parts of the interior of the structure in perishable materials.

It was hoped that, through the adoption of the compromise propositions, the importance of the active operations would speedily become apparent, and that the plan of erecting an expensive building would be abandoned before more than one of the wings had been completed; but, though the construction of the edifice was, in accordance with the agreement, extended over a number of years, yet in anticipation of such an interference with its ultimate completion, so large a portion of the lower story of the whole structure was commenced in the first two years that it was apparent no successful opposition could be made to its further progress. Nor can Congress be absolved of the charge of having indirectly contributed to encumber the bequest with the cost and maintenance of so extensive a building and so numerous a retinue: with more justice, therefore, may it be invoked to relieve the Institution, in due time, from the burden imposed upon it. It should, however, be remembered, on the other hand, that by repeated enactments Congress has sanctioned the prominence which has been given to the active operations, and acquiesced in the adoption of the special character which has been impressed on the library and museum. It has relieved the Institution from the care of the grounds, also of the copyright books which were intended to swell the number of volumes, and, so far from still considering the museum of the exploring expedition a desirable gift, it has granted for several years past four thousand dollars annually to assist in bearing the expenses of preserving and exhibiting the specimens.

It is to be regretted that Congress directed that provision should be made on a large scale for a library and museum, since each tends to cripple the other, and the whole to diminish the efficiency of the active

operations. A conscientious endeavor, however, has been made to harmonize the whole scheme, by establishing a special library, consisting of the transactions of learned societies and systematic works on all branches of science, together with a limited museum of type specimens, principally of the products of the American continent. And, on the whole, it may be pronounced that, notwithstanding the inauspicious circumstances which attended the commencement of the Institution, as before stated, and the difficulties with which it has had to contend from time to time, the results it has produced have been such as to commend it to the public generally throughout our own country, and to make it favorably known to the cultivators of science wherever found. It has identified itself with the history of almost every branch of knowledge which receives attention at the present day, and its transactions and proceedings are constantly referred to as authoritative on all subjects to which they pertain. With no desire to exaggerate its importance or advantages, the fact may be satisfactorily cited that the recognition of its services in behalf of science exists in the contemporary works of all languages, that its publications are found wherever letters are cultivated, and its specimens in all the principal museums of the world. If it was the desire of the founder to perpetuate the memory of his liberality, that desire has been thus fully gratified; nor is the memorial of his enlightened and comprehensive benevolence limited as to place or time, since it is everywhere renewed with the yearly dissemination of the publications which bear his name.

The following brief sketch of the labors of the Institution up to the present time will not only serve to show what it has done, but also to illustrate the capability of the plan of active operations for producing important results in the way of increasing and diffusing knowledge among men.

ACTIVE OPERATIONS.

Publications.—The Smithsonian Institution has established three classes of publications, in which are contained the articles hereafter to be mentioned. These are as follows:

1. A quarto series, entitled "Smithsonian Contributions to Knowledge," issued in volumes, each embracing one or more separate articles. Of these the fourteenth is nearly through the press.

2. An octavo series, entitled "Smithsonian Miscellaneous Collections," which in the aggregate make six large volumes.

3. Another octavo series, consisting of the annual reports of the Institution to Congress, called "Smithsonian Reports," of which eleven volumes have been published.

The Smithsonian Contributions to Knowledge include memoirs embracing the records of extended original investigations and researches resulting in what are believed to be new truths, and constituting positive additions to the sum of human knowledge.

The series of Smithsonian Miscellaneous Collections contains reports on the present state of our knowledge of particular branches of science; instructions for collecting and digesting facts and materials for research; lists and synopses of species of the organic and inorganic world; museum catalogues; reports of explorations; aids to bibliographical investigations, &c.; generally prepared at the express request of the Institution, and at its expense.

The Annual Reports include the official reports of the Secretary to the Board of Regents of the operations and condition of the Institution; the reports of committees of the board; abstracts of lectures delivered before the Institution; extracts from correspondence; original or translated articles relating to the history and progress of science, &c.

The following rules have been observed in the distribution of the first and second series:

1. They are presented to all learned societies of the first class which publish transactions, and give copies of these, in exchange, to the Institution.

2. To all foreign libraries of the first class, provided they give in exchange their catalogues and other publications, or an equivalent, from their duplicate volumes.

3. To all the colleges in actual operation in this country, provided they furnish, in return, meteorological observations, catalogues of their libraries and of their students, and all other publications issued by them relative to their organization and history.

4. To all States and Territories, provided they give, in return, copies of all documents published under their authority.

5. To all incorporated public libraries in this country, not included in any of the foregoing classes, now containing 10,000 volumes; and to smaller libraries, where a whole State or large district would be otherwise unsupplied.

Institutions devoted exclusively to the promotion of particular branches of knowledge receive such articles published by the Institution as relate to their objects. Portions of the series are also given

to institutions of lesser grade not entitled, under the above rules, to the full series, and also to the meteorological correspondents of the Institution.

The reports are of a more popular character, and are presented—

1. To all the meteorological observers and other collaborators of the Institution.
2. To donors to its library or museum.
3. To colleges and other educational establishments.
4. To public libraries and literary and scientific societies.
5. To teachers or individuals who are engaged in special studies, and who make direct application for them.

Besides the works which have been published entirely at the expense of the Institution, aid has been furnished by subscription for copies to be distributed to foreign libraries of a number of works which fall within the class adopted by the programme. The principal works of this kind for which subscriptions have been made are as follows: Agassiz's Contributions to Natural History, Gould's Astronomical Journal, Shea's American Linguistics, Runkle's Mathematical Monthly, Deane's Fossil Footprints, Tuomey & Holmes's Fossils of South Carolina, Peirce's Analytic Mechanics.

Meteorology.—The investigation of all questions relative to meteorology has been an object to which the Institution has devoted special attention, and one of its first efforts was to organize a voluntary system of observation, which should extend as widely as possible over the whole of the North American continent. It induced a skilful artisan, under its direction, to commence the manufacture of carefully prepared and accurately graduated instruments, now generally known as the Smithsonian standards. It prepared and furnished a series of instructions for the use of the instruments and the observations of meteorological phenomena; also three series of blank forms as registers.

It next organized a body of intelligent observers, and in a comparatively short time brought the system into practical operation; each year the number of observers increased, and where one ceased his connexion with the enterprise, several came forward to supply his place. By an arrangement with the Surgeon General of the army, the system of observations at the United States military posts in different parts of the country, and also that which had previously been established by the State of New York, were remodelled so as to harmonize with that of the Institution. Gentlemen interested in science, residing in the British provinces, and at nearly all the posts of the Hudson's Bay Company, also in Mexico, Central America, the West In-

dies, and some places in South America, &c., joined in the enterprise; and, with few exceptions, at the beginning of the war every district of considerable size had in it at least one if not more observers. All these contribute their services without compensation, their only reward being the satisfaction of co-operating with each other and the Institution in the effort to supply data and materials for investigation. Any returns, indeed, which the Institution has in its power to make are gladly rendered in a hearty acknowledgment of assistance, and in copies of all the Smithsonian publications likely to be of interest.

Besides the materials obtained directly from the observers of the Institution, a large amount of other matter relative to the meteorology of North America has been accumulated—such as copies of all the known series of records for long periods which could be obtained; series which have been compiled during explorations and surveys for the government, those which have been the result of local associations, and of the system of observations established in connexion with the survey of the great lakes, as well as of the common school system of Canada, and many thousand notices of the weather at different times and places, collected from newspapers and periodicals.

No other part of the world has offered such facilities for the collection of meteorological data, the system extending over so large a portion of the earth's surface; the observers, with few exceptions, all speaking the same language, and many of them being furnished with full sets of compared standard instruments.

It is to be regretted that this system has been partially interrupted during the war, and that the portion of the income of the Smithsonian fund, which could be devoted to the reduction and discussion of the material collected, has not been adequate to the labor of deducing from so large a body of data all the valuable truths which they are capable of affording. It has had assistance, however, from the agricultural department of the Patent Office, by which the results of five years' observations of all the elements and a series of temperatures for long periods have been prepared for publication.

From all the observations made up to 1860, isothermal charts were constructed, presenting much more accurately than had ever been done before, the distribution of temperature over the continent of North America; a series of rain charts, and also a large map exhibiting the regions of original forest, of arable prairie and of desert in the United States, have also been prepared.

The Institution has fully established the fact, which was previously

indicated in regard to storms, by the investigations of Mr. Espy and others, in relation to the United States, namely, that all such meteorological phenomena, as variations in the pressure of the atmosphere, sudden changes of temperature, either of unusually warm or cold weather, thunder-storms, tornadoes, as well as storms of wind, rain, &c., which occur within the temperate zones, travel from west to east. The simultaneous system of observations established by the Institution furnished the means of placing this great law of meteorology in prominent relief, and of first reducing it to practical utility.

As early as 1849 the Institution organized a system of telegraphic despatches, by which information was received at Washington of the condition of the weather at distant places in the southwest and northwest, and from this, in accordance with the law before mentioned, it was often enabled to predict, sometimes a day or two in advance, the approach of any larger disturbances of the atmosphere. Subsequently the telegraphic despatches were daily exhibited at the Institution on a map of the United States by means of a series of movable cards of different colors, which indicated the meteorological condition at various points, showing at a glance in what parts of the country it might be clear or cloudy, raining or snowing; and by arrows the existing direction of the wind. The returns were also published in one of the evening papers. Unfortunately this enterprise was interrupted by the cessation of the observations in the southwest, and by the constant use of the telegraph for the purposes of the government.

The advantages possessed by the Smithsonian Institution for investigations of this kind will be evident, when it is recollected that a large portion of its observers are stationed west of Washington, that the phenomena approach it over a large extent of land, and can be critically noted through every part of their passage eastward, while the phenomena which are presented to the meteorologists of Europe traverse in reaching them a wide expanse of ocean, from which only casual observations can be gleaned.

The publications of the Institution contain many memoirs which have tended to advance the science of meteorology. Among these may be mentioned the meteorological and physical tables prepared at the expense of the Institution by Professor Guyot, and filling a large octavo volume of the Miscellaneous Collections. No work extant answers the same purpose with the one referred to, which has hence become a general standard of reference, the constant demand for it

as well in Europe as America having required the printing of several successive editions.

The results of the reductions for five years previous to 1860 have been published in two volumes of nearly 2,000 quarto pages, containing a mass of materials of great value in determining the average temperature, fall of rain, barometrical pressure, moisture, direction of the wind, and time of various periodical phenomena relative to plants, animals, &c.

In addition to these large and important volumes, other works have been published by the Institution which have had a marked influence on the progress of meteorology. Among these may be mentioned the works of Professor Coffin, on the winds of the northern hemisphere; of Mr. Chappelsmith, on a tornado in Illinois; of Professor Loomis, on a great storm which pervaded both America and Europe; the reduced observations for twenty-eight years of Professor Caswell, at Providence, Rhode Island; of Dr. Smith, for twenty years in Arkansas; of Dr. Kane and Captain McClintock, in the arctic seas; on the heat and light of the sun at different points, by Mr. Meech; on the secular period of the aurora, by Professor Olmsted; the occurrence of auroras in the arctic regions, by Mr. P. Force, &c.

Besides these, a series of meteorological essays embodying many of the results obtained from the investigations at the Institution has been prepared by the Secretary, and been published in the agricultural reports of the Patent Office.

Astronomy.—The Institution has advanced the science of astronomy both by its publications and the assistance rendered to observers. To facilitate astronomical observations, it prepared and published for six years an annual list of occultations of the principal stars by the moon, and printed and distributed a series of tables for determining the perturbations of the planetary motions, the object of which determination is to facilitate the calculation of the places of the heavenly bodies. These tables have accomplished the desired end, saving to the practical astronomer an immense amount of tedious and monotonous labor.

The name of the Institution has been favorably connected with the history of the interesting discovery of the planet Neptune. From a few of the first observations which had been made on this planet Mr. Sears C. Walker calculated its approximate orbit, and by this means tracing its path through its whole revolution of 166 years he was enabled to carry it backward until it fell among a cluster of stars, accurately mapped by Lalande, towards the close of the last century.

After minute inspection he was led to conclude that one of the stars which had been observed by Lalande in 1795 was the planet Neptune. He was thus supplied with the amount of its motion for upwards of fifty years, from which he deduced a much more perfect orbit, and was enabled to construct an ephemeris giving the place of the planet for several years in succession. These investigations, so interesting to astronomy and honorable to this country, were prosecuted and published at the expense of the Institution, the name of which will be further connected with the planet Neptune by the publication, now in press, of a new discussion of all the observations which have been made on this body for the last fifteen years. This work, which is by Professor Newcomb, of the United States navy, will furnish not only the means of determining the exact position of Neptune for years to come, but also the data for ascertaining whether it is affected by other bodies than those now known to astronomers.

To render more generally accessible to practical astronomers in this country the theory of the motion of the heavenly bodies by the celebrated Gauss, the Institution shared the expense of publishing a translation of this treatise by Admiral C. H. Davis, U. S. N., from the Latin. It furnishes a complete system of formulas for computing the movements of a body in any of the curves belonging to the class of conic sections, and a general method of determining the orbit of a planet or a comet from three observations, as seen from the earth.

For a number of years aid was afforded to the publication of Gould's American Astronomical Journal, which rendered good service to the science by making promptly known to foreign observers the results of the labors of their contemporaries in America. It has also had reduced by Mr. Charles A. Schott, and published at its own expense, the astronomical observations made by Dr. Kane in the arctic regions, and has now in hand those which were made in the same regions by Dr. Hayes.

Congress having authorized in 1849 an astronomical expedition under Lieutenant Gilliss to the southern hemisphere for the purpose of determining the parallax of the planets, and consequently their distance from the sun, by observations on Venus and Mars, accidentally failed to make the appropriation for instruments. This omission was supplied by the Institution, which was subsequently indemnified for the expense by the Chilian government.

In the observation of all the larger solar eclipses which have happened since the date of its organization the Institution has actively

and efficiently co-operated by publishing projections of the phases and times of their occurrence in different parts of America.

Under its auspices, and partly at its expense, an expedition was inaugurated by Lieutenant Gilliss to observe the great eclipse of 1858 in Peru, from which data of value for the improvement of solar and lunar tables were determined, besides facts of interest in regard to the physical constitution of the sun.

Assistance was also rendered to the expeditions under the direction of the Coast Survey to observe the eclipse of July 18, 1860, one of which was sent to Labrador, under the charge of Professor S. Alexander, of New Jersey, and the other to Washington Territory, under that of Lieutenant Gilliss.

To these may be added an account of an instrument invented by Rev. T. Hill, president of Harvard College, for the projection of eclipses.

Physics and chemistry.—The Institution has fostered these sciences in many different ways; among others, by importing models of the most improved articles of apparatus, and making them known to scientific men through lectures and otherwise.

It has instituted an extensive series of experiments on building materials, particularly in reference to those employed by the government in the construction of the Capitol and other public edifices; also a like series on acoustics, as applied to public halls, and the principles deduced from these were practically applied in the construction of a model lecture-room. It has made a very extended series of experiments on different substances employed for light-house illumination, from which has resulted the substitution of another material for sperm oil, and the consequent annual saving of a large amount of money to the government.

In compliance with requests made by different departments of the government and of Congress, particularly since the war, it has conducted various series of investigations, principally in relation to questions involving mechanical, chemical, and physical principles, and has made reports on subjects of this kind amounting, in the aggregate, to several hundred.

To facilitate researches, a laboratory has been established and kept constantly in working condition, the privilege of using it having been given to various competent persons for experimenting in different branches of physical science. Just now it is occupied by Dr. Wetherill for the purpose of conducting a series of analyses of samples of

air from the halls of Congress, &c., from which a report is to be made, under the direction of the Institution, on the ventilation of the public buildings of this city.

The most important publications under this head are the researches relative to electric currents, by Professor Secchi; on the explosibility of nitre, by Dr. Hare; on the ammonia-cobalt bases, by Drs. Gibbs and Genth; and on astronomical photography, by Dr. Henry Draper.

A valuable report on recent improvements in the chemical arts by Booth & Morfit was published in 1852, and there have been given in the annual reports of the Institution a series of translations and articles presenting a view of the progress of physics and chemistry from year to year, since 1853, among which we may particularly notice the translation of Müller on recent contributions to electricity, and the reprint of Powell on Radiant Heat.

Terrestrial magnetism.—The subject of terrestrial magnetism has been prosecuted simultaneously with that of meteorology, and an observatory was erected in the Smithsonian grounds, fitted up with the most approved instruments, and conducted under the joint auspices of the Institution and of the Coast Survey. After remaining in operation for several years, the instruments were transferred to Key West, as a remote station where observations were still more desirable. Instruments were also furnished an expedition to Mexico, and used with much success by Mr. Sonntag, whose results were published in the Smithsonian Contributions to Knowledge. Apparatus was also furnished to Dr. Kane, Dr. Hayes, and other explorers, by means of which valuable results were obtained.

Of the more important publications of the Institution, which have tended to advance this science, may be mentioned the articles by Dr. Locke, on the dip and intensity; the elaborate discussion, by Professor Bache, of the magnetic observations made at Girard College from 1841 to 1845; the report on magnetical observations in the arctic seas by Dr. Kane, reduced at the expense of the Institution by Mr. C. A. Schott, and those made in Pennsylvania and adjacent States by Professor Bache, and in Mexico by Mr. Sonntag.

Explorations.—In the deficiency of means for more extended operations, as has been frequently represented in the annual reports, the efforts of the Institution in the line of explorations and collections are confined, as strictly as possible, to America; but within this limit there are few regions which have not furnished scope, in some form, to its activity. Arctic America, all the unknown portions of the

United States, Mexico, Central and South America, and the West Indies, have been laid under contribution for facts and materials by which to advance science.

An eminently useful influence has been exerted by the Institution through the aid it has afforded in the organization of the different government explorations by land and by sea. Whether by official representations to the heads of departments, or personal influence with officers and employés, it has secured the engagement of individuals competent to collect facts and specimens; it has instructed persons thus engaged, and others, in the details of observation; it has superintended the preparation, and, in some cases, borne the expense of the necessary outfits; has furnished fresh supplies from time to time to the collectors while in the field; received the collections made, and preserved them for future study, or at once consigned them to the hands of competent persons, both at home and abroad, for investigation; directing the execution of the necessary drawings and engravings for the reports, and, finally, superintending the printing and even the distribution of any available copies of the completed works to institutions of science. Prior to the establishment of the Institution but little had been done by our government in the way of scientific explorations,. with the exception of that under Captain Wilkes. But since then nearly every United States expedition, whether a survey for a Pacific railroad route, a boundary line between the United States and regions north or south of it, or within its borders, a wagon-road across the Rocky mountains, or an ordinary topographical exploration, has been influenced and aided more or less, as above stated. A list of the expeditions has been, from time to time, published in the annual reports, and it is sufficient here to say that their total number up to the present time is about fifty.

Besides these, similar explorations have been carried on without any reference to the government, and either entirely or in a great measure at the expense of the Institution, and always at its suggestion, or under its direction. Prominent among these may be mentioned the three years' researches in the arctic regions, by Mr. Kennicott, with the co-operation of gentlemen of the Hudson's Bay Company; of Mr. Drexler, in the region of Hudson's bay, and also in the Rocky mountains; of Mr. Coues, in Labrador; of Lieutenant Feilner, in Nebraska and Northern California; of Mr. John Xantus, at Fort Tejon, Cape St. Lucas, and in Western Mexico; of Lieutenant Trowbridge, on the coast of California; of Drs. Cooper and Suckley, in Western America generally; of Drs. Coues and Beers, in Kansas,

New Mexico, and Arizona; of Dr. Irwin, in Arizona; of Dr. Hitz, about Laramie Peak; of Lieutenant Couch, in Texas and Mexico; of G. Wurdemann, Lieutenant Wright, Captain Woodbury, and others, in Florida and the Gulf of Mexico; of Dr. Sartorius, Professor Sumichrast, Dr. Berendt, in Mexico; Dr. Von Frantz, J. Carniol, in Costa Rica; of Mr. March, in Jamaica; of Mr. Wright, Dr. Gundlach, Professor Poey, in Cuba; Judge Carter, in Bolivia, besides many others.

In addition to the collections which have been received from explorations organized under the direction of the Institution, large numbers of duplicate specimens have been presented by the meteorological observers and other Smithsonian collaborators, the whole forming a body of material for the illustration and study of the products of the American continent unequalled by any collection previously made. The explorations, however, as might be inferred, have not been confined to the collecting of specimens, but have also furnished information relative to the topography, geology, physical geography, ethnology, and the living fauna of the regions visited.

The results have been published by government, the Institution, or other parties. The extent and importance of these publications may be seen in the volumes of the reports of the Pacific railroad and Mexican boundary surveys; of the United States astronomical expedition to Chili, under the late lamented Captain Gilliss; of Captain Stansbury's exploration of Utah; of Lieutenant Michler's of the Isthmus of Darien, &c., &c.; in the volumes of the Smithsonian publications, and in the transactions of nearly all the scientific institutions in the United States.

In order to facilitate the operations of collectors, a series of general directions have been prepared and widely distributed, free of charge, for collecting, preserving, and transporting specimens of natural history, and also special instructions for collecting nests, eggs, shells, insects, &c.

Description and distribution of collections and specimens.—The object of making these collections, in conformity with the policy of the Institution, was not merely to supply a large museum in Washington with permanent specimens or duplicates for exchange, but to furnish the naturalists of the world with the materials for advancing the science of the natural history of North America, and of facilitating the study of its various branches by supplying museums both in the United States and in Europe with sets of type specimens.

In pursuance of this object, full sets of the specimens collected have been submitted to a large number of naturalists, both in this country and abroad, for critical study and description, and it is not too much to say that scarcely a monographic investigation has been conducted for ten years past in any branch of American zoology which has not derived part or the whole of its material from the Smithsonian collections. Duplicates of the specimens, when described, have been made up into series for distribution, always accurately labelled, and are usually types of some published investigation. The average of such distribution has, for the last ten years, been at least ten thousand specimens annually, while the distribution of 1864 amounted to nearly five thousand species and seventeen thousand specimens. In this way, besides supplying the principal museums of Europe with specimens, all the older museums in this country as well as Canada have been largely increased, and the foundation for several new establishments of a similar kind has been furnished. As an illustration of what has been done in the way last mentioned, I may cite the large donation of labelled specimens which has been made to the museum of the University of Michigan, and the co-operation which has been afforded the liberal-minded citizens of Chicago in founding a museum and establishing a society of natural history, which, under the direction of Mr. Kennicott and Dr. Stimpson, is diffusing a taste for the study of nature in that city of unparalleled growth, which cannot be otherwise than highly salutary in ameliorating the sensual effects of great material prosperity.

The Institution has also done good service in promoting and assisting the formation of local societies in rural districts for the collection of specimens and the recording of natural phenomena. To all societies of this kind, as well as to colleges and academies making special application, labelled specimens have been presented.

This distribution of specimens is very different from the ordinary exchanges conducted between institutions or individuals, which usually involve the return of an equivalent. The question with the Smithsonian Institution is, not what can be had in return, but where a particular specimen or series of specimens can be placed so as best to advance the cause of science, by being most accessible to the largest number of students engaged in original investigations.

Palæontology, geology, physical geography, &c.—Appropriations have been made for investigations of the surface formation of the Connecticut valley by Professor E. Hitchcock, and for the collection of materials for the illustration of the geology and palæontology of par-

ticular regions. Appropriation has also been made to Professor Guyot for a barometrical survey of the different parts of the Alleghany mountains, and to other persons for collecting observations on heights, as determined in different parts of the country by the various canal and railway surveys.

The publications on these subjects, besides the papers of Professor Hitchcock on surface geology, are as follows:

A memoir on *Mosasaurus*, by Dr. R. W. Gibbs.

On the extinct species of the fossil ox and sloth of North America, and on the ancient Fauna of Nebraska, by Dr. Leidy.

On the Physical Geography of the Mississippi Valley, by Charles Ellet.

On the Law of Deposit of Flood Tide, by Admiral Davis.

On the Fluctuations of the level of the great American Lakes, by C. Whittlesey.

On the Palæontology of the Upper Missouri, and Check List of miocene, cretaceous and jurassic Invertebrata, by F. B. Meek.

A memoir by Dr. Leidy, now in press, on the extinct reptiles of the cretaceous period, will, it is believed, be a valuable manual of reference.

The Institution has published a Check List of minerals, with their symbols, prepared by Mr. Egleston, with special reference to facilitating the labelling of the Smithsonian minerals and the exchange of specimens, and it may be mentioned that extensive distribution has been made of specimens of building stone employed by the government.

Botany.—This branch of general natural history has been advanced by the Institution, not only by means of the publication of original memoirs, but also by explorations and collections made at the expense of the Smithsonian fund. The most important work which has been published is a large quarto volume, illustrated by expensive colored plates, on the algae of the entire North American waters. The work was written for the Institution by Dr. Harvey, of the University of Dublin, and has been the means of rendering this order of the vegetable kingdom more generally known. The Institution has also published several papers on the plants of New Mexico and California, by Dr. Gray, of Cambridge, and Dr. Torrey, of New York.

Duplicates of the specimens described have been presented to institutions at home and abroad. Considerable labor has also been ex-

pended in the preparation of an original report on the forest trees of America, by Dr. Gray. This work, however, has been interrupted for some time, but will be resumed, it is expected, during the present year.

General Zoology.—A large part of the collections made by the Institution belong to the general class of zoology, intended to advance the study of animal life upon the continent of America.

The ornithology of America has always been a speciality of the Smithsonian Institution, more efforts having been made to perfect its collection in this department than any other. The Institution has published the first part of a work by Dr. T. M. Brewer, suitably illustrated, on the distribution and habits of North American birds during the breeding season, with descriptions and figures of their eggs, the materials being derived entirely from the collections of the Institution, and mostly made at its special request. This is the first separate work on North American zoology ever prepared. A catalogue of North American birds, prepared by Professor S. F. Baird, has been extensively used at home and abroad in labelling collections.

Professor Baird is now engaged in preparing a general report on our knowledge of North American ornithology to the present date, with the addition of the species of Central and South America and the West Indies; the materials being derived almost entirely from the specimens collected by the Institution, which have been increased since the publication of the extensive work on the same subject by Professor Baird in the Pacific railroad report, from 12,000 to 35,000.

The collections which have been made by the Institution for the illustration of mammalia have been very extensive, amounting to 6,000 specimens, and have not only included many duplicates of the species previously known, but a very large number entirely new to science. A catalogue of North American mammals, chiefly those collected by the Institution, prepared by Professor Baird, has been published and distributed to those interested in the study; also a monograph of North American bats, prepared by Dr. H. Allen. Materials are now in course of accumulation to complete the account of the classes of mammals of North America which have not been included in the publications of the Institution and Pacific railroad reports.

As with all American vertebrata, the collections of reptiles and fishes made by the Institution have been very extensive, and numer-

ous monographs or articles have been published relative to them in the Pacific railroad reports and the proceedings of different natural history societies, the Institution having published a synopsis of the serpents of North America, and a monograph of the Cottoids.

The Institution has materially aided the study of the entomology of this country, not only by the collections in that branch, but by preparing and publishing a series of works for the purpose of exhibiting the state of knowledge on the subject and facilitating its further advancement. It has published and distributed the following under this head:

Instructions for collecting and preserving insects, and catalogues, synopses, or monographs of the Diptera, Coleoptera, Lepidoptera, and Neuroptera, prepared by the most competent authorities in Europe and America.

It has also in course of preparation works relative to the Hymenoptera, Homoptera, Hemiptera, Orthoptera, &c.

In the preparation of these publications the Institution is indebted for gratuitous assistance to Dr. Jno. Leconte, Baron Osten Sacken, and others.

Conchology.—A large collection of specimens of shells was received from the United States exploring expedition, which has been much increased by subsequent additions. All the shells of the west coast of the United States, and those generally collected by the exploring expedition, have been put into the hands of Mr. P. P. Carpenter, of England, the new ones to be described for publication, and the duplicates of the whole to be arranged for distribution to museums, colleges, and other establishments. This work is nearly completed, and a large number of partial sets of the shells have been distributed in accordance with the plan just mentioned. The publications on this subject are lists of North American shells, circulars relative to collecting, an elementary introduction to the study of conchology, and an extensive work in two octavo volumes on the Bibliography of North American Conchology, by W. G. Binney, and a monograph of the Corbiculadæ, by Temple Prime. Besides these a number of articles are in the press or in course of preparation.

Microscopy.—Encouragement has been given to this branch of science by importing, as samples, simple forms of working microscopes, and also by stimulating our native artists to greater exertion in the construction of this instument, by ordering the best that could be produced. Samples of microscopic organisms have been collected

and distributed to observers, and examinations and reports have been made on a large number of this class of objects sent to the Institution. The publications in regard to this subject are a number of papers by Professor Bailey, of West Point, and a very interesting memoir by Dr. Leidy, of Philadelphia, on a fauna and flora within living animals.

Physiology.—No experiments on this subject have been made under the immediate direction of the Institution, although it has furnished the materials for investigation by other parties. The publications in regard to it are chemical and physical researches concerning North American vertebrata, by Dr. J. Jones; researches upon the venom of the rattlesnake, with an investigation of the anatomy and physiology of the organs concerned, by Dr. S. W. Mitchell; on the breathing organs of turtles, by Drs. Mitchell and Morehouse; on the anatomy of the nervous system of rana pipiens, by Dr. J. Wyman; and on the medulla oblongata by Dr. John Dean.

Ethnology and Philology.—One of the earliest efforts on the part of the Institution was directed to the advancement of the science of American ethnology. Its first publication as well as introductory volume to the series of Smithsonian Contributions to Knowledge, being the work of Squier and Davis, on the ancient monuments of the Mississippi valley, remains the standard treatise on this subject. This was followed by a similar work on the antiquities of New York, by Mr. Squier; and those of Wisconsin, by Mr. Lapham, of Ohio; and of Lake Superior, by Mr. Whittlesey; a memoir on some antiquities of Mexico, by Brantz Mayer; and a general introduction to the whole subject of American archæology, by Mr. Haven, besides many articles of less extent in one or another of the Smithsonian series. Several pamphlets of instructions for making observations and collections in this science have also been issued.

In the department of philology, also, the Institution has evinced its zeal and activity by the publication, among others, of the elaborate work on the Dakota language, by Mr. Riggs; that on the Yoruba language, by Mr. Bowen; and that on the Chinook jargon, by Mr. Turner and Mr. Gibbs. To Mr. Shea, of New York, who is engaged in the preparation of a library of American languages, annual appropriations from the funds of the Institution have been made in furtherance of the publication of linguistic memoirs furnished by its correspondents.

Systematic efforts have been directed by the Institution to the collection of as perfect a series as possibleof the specimens of Ameri-

can antiquities, and of those illustrative of the habits of the modern native tribes. Already an extensive collection has been accumulated, and the preparation and distribution of a series of colored casts of the more interesting specimens of aboriginal art have been commenced. The former picture gallery had just been fitted up with cases two hundred feet in length, for the reception of these, when the disastrous fire occurred, which destroyed the upper part of the centre building; fortunately, however, before any of these specimens had been placed in the room.

Correspondence.—The Institution has constantly received a large number of communications, asking information on a variety of subjects, particularly in regard to the solution of scientific questions, the names and characters of objects of natural history, and the analysis of soils, minerals, and other materials which pertain to the industrial resources of the country. Answers have in all cases been given to these inquiries, either directly by the officers of the Institution or by reports from the Smithsonian collaborators. A considerable portion of the correspondence burned in the office of the Secretary was of this character. The loss in this case is to be regretted, not only on account of the valuable information the letters and answers contained, but also on account of the illustration they afforded of the influence of the Institution, and the condition of the public mind at a given time. Every subject connected with science which strongly attracts popular attention never fails to call forth a large number of inquiries and suggestions.

International exchanges.—To facilitate the direct correspondence between the learned institutions and scientific men of the two worlds, and the free exchange of their publications, has, from the first, been a special object of attainment with the Smithsonian Institution. Year by year its plans for this purpose have been modified and improved, until the system has become as nearly complete and satisfactory as the funds and force at its disposal will allow. At the present day it is the great medium of scientific intercommunication between the New World and the Old; its benefits and services being recognized alike by individuals, institutions, and governments. Its parcels pass all the custom-houses without question or interference, while American and foreign lines of transportation, with rare exceptions, vie with each other in the extent of the privileges accorded it. To so great an extent has its sphere of activity been enlarged, that it is no exaggeration to say that a very large proportion of all international exchanges of the kind referred to are now made through its instrumentality.

At the present time the Institution is prepared to receive, at periods made known through its circulars, any books or pamphlets of scientific, literary, or benevolent character which any institutions or individuals in America may wish to present to a correspondent elsewhere, subject only to the condition of being delivered in Washington free of cost, and of being accompanied by a separate list of the parcels sent. Where any party may have special works to distribute, the Institution is always prepared to furnish a list of suitable recipients. In many cases where works of value have been published by the United States or State governments, likely to be of importance to students abroad, application has been made by the Institution for copies, in most cases with success. The articles and volumes, when received, are assorted and combined into packages, and these, after being properly addressed and enclosed in boxes, are despatched to the agents of the Institution in London, Leipsic, Paris, and Amsterdam. The boxes are there unpacked, and the contents distributed through the proper channels; the returns for these transmissions are received by the same agents, and boxed, and forwarded to Washington, from which point the parcels for other parties are sent to their proper destination. All the expenses of packing, boxing, agencies, freights, &c., are borne by the Institution, with the exception of the local conveyance of single parcels by express or otherwise within the United States.

LOCAL OBJECTS.

Under this head we have classed those parts of the programme which were indicated by Congress, and which do not, so directly as the objects we have already described, contribute to the advance of knowledge. It will be seen, however, that they have been made as far as possible to harmonize with the active operations, and to assist in their progress.

Library.—Although the act of Congress directed that provision should be made for the accommodation of a library, on a liberal scale, it was soon seen, after the organization of the Institution, that it would be impossible, from the income which could be devoted to it, to establish a first-class general library. Even had this been practicable, it would still have seemed superfluous to do so in the very vicinity of the miscellaneous library of Congress, which is every year increasing in extent under the liberal appropriations which are annually made for the purchase of books. It was therefore deemed

preferable, and more consonant with the purposes of the Institution, to form a special library, which might constitute, as it were, a supplement to the library of Congress, and consist, for the most part, of complete sets of the proceedings and transactions of all the learned societies in the world, and of other serials essential for reference by students specially engaged in original scientific research. The efforts of the Institution to carry out this plan, which has since been sanctioned by Congress, have been eminently successful. Principally through exchanges, and occasionally by purchase, a more complete collection of the works above mentioned has been procured than is to be found in any library of the United States, or is easily met with even in Europe. The Institution has been assisted in making this collection by the liberality of many of the older libraries abroad, which, on application, have furnished from their duplicates volumes and even whole sets to complete series of works long since out of print, and which, in some cases, could not have been obtained through any other means. The library is also quite rich in monographic or special treatises in the physical and natural sciences, lacking as yet, it is true, some of the more expensive volumes, but still affording the means of prosecuting almost any scientific investigation. One specialty consists of the large number of maps and charts obtained by exchange from geographical and hydrographical establishments, &c. This collection is as complete as any in the country.

No effort is spared to render the library of the Institution conducive to the advance of science. Two editions of the catalogue of serial works have already been published, and a third is now in press; this will probably fill four hundred octavo pages, and will be completed in the course of the present year, to be followed by a catalogue of the special works.

As in most libraries of special character, and, indeed, in most large public libraries, the public are allowed free access to the library-room during office hours, but are not generally permitted to take books away. When, however, any applicant is known to be engaged in the prosecution of original investigations, which promise to advance science, and requires the assistance of books found in the Smithsonian library, they are freely lent, even to persons in the remote portions of the United States. Any losses which may occur by the adoption of this course are more than compensated by the advantages derived from it.

Congress had provided by the law of organization that a copy of all copyright works should be presented to the library of this Institu-

tion. This it was supposed would be the means of securing important additions to the library. It was found, however, in practice, to impose a burden on the funds of the Institution for which no adequate compensation accrued; copies of the most valuable works were not presented, because there was no penalty imposed for the neglect to comply with the requirement, and the expense of clerk-hire in recording and furnishing certificates was greater than the value of the articles received, consisting, as they did principally, of sheets of music, labels of patent medicines, novels, and elementary works of instruction. The law was, therefore, on special application, so modified that authors were required in future only to send a copy of their works to the copyright bureau of the Department of the Interior and to the Library of Congress.

A special library of the character above described, consisting of serials, must of necessity constantly increase with the additions made to the series of the existing associations which annually publish their transactions. The Smithsonian library, therefore, comprises a principle of indefinite augmentation, both as regards extent and value; and although this increase will result mainly from the exchanges produced by the active operations, yet additional accommodations will be constantly acquired. Hence it may become a matter of consideration, hereafter, whether, since Congress has appropriated $160,000 to the enlargement of the accommodation for its own library, it may not be expedient to request that the Smithsonian collection be received and arranged as one of its departments, while the free use and general control of the same shall still be retained by the Institution.

Museum.—The same remarks which have been made in regard to the library may, with little modification, be applied to the museum. The portion of the funds of the Institution which it is practicable to devote to the museum is not sufficient to support an establishment of this kind worthy of the seat of government of the United States. Indeed, it is generally now conceded by those who have critically examined the subject, that the accommodation and perpetual maintenance of a large collection of objects of nature and art intended for popular exhibition, or even for educational purposes, ought not to have been imposed upon the Smithsonian fund. It has been seen from the foregoing statement how much can be done in the way of advancing natural history independent of a costly edifice, and the support of a popular museum in which are to be continually exhibited even type specimens. It is true that specimens of this character ought

to be preserved for study; but seeing that there are in the country a number of special museums which would gladly become the custodians of these objects, and that the hope is yet confidently entertained that Congress will, in due time, establish a national museum which shall rival those of other countries, it has been thought advisable to restrict the collections which are retained in the Smithsonian museum—first, to those made by the exploring expedition, the care of which Congress has devolved upon the Institution; and, second, to such type specimens as are thought of special interest as illustrating the Smithsonian publications.

The museum has been rendered particularly attractive to the visitors and inhabitants of Washington by the large number of birds and mammals which have been mounted for public exhibition, and in this way it has undoubtedly contributed to the popularity, though it has diminished the efficiency, of the Institution. The danger, however, to be guarded against, is the constant tendency to expand the collections, and hence gradually to absorb the income in their support. It should be recollected that the building has borne upon the resources of the Institution with a cost of more than $300,000, and that at least an additional $100,000 will be necessary to repair the recent damages, and this mainly to render the edifice better adapted for the accommodation of the library and museum.

Little has been said in this sketch in regard to the gallery of art. The impropriety of expending the income of the bequest in attempting to form a collection of articles in this line worthy of the country has had no prominent advocates, even among artists; still, in connexion with the museum, a collection has been formed which principally consists of plaster casts of distinguished individuals, and a few pictures which have either been presented to the Institution or are the property of the government. The only purchase in this line which the Institution has made is that from Hon. George P. Marsh, of a series of valuable engravings to illustrate the early history of art.

Lectures.—As a part of the programme of organization finally adopted, courses of lectures were to be delivered, but instead of attempting to furnish popular instruction by this means to all parts of the country, as was at first proposed, the lectures have been confined to the city of Washington; and in order to render them generally useful, synopses of the more important ones have been published in the annual reports. At the commencement of the Institution, and

before the plan of organization was generally understood, special care was taken to invite as lecturers men of prominence in the line of literature that they might have an opportunity to become familiar with the plan adopted, and in this way many prejudices were removed and much information diffused as to the character of the establishment.

The lectures were commenced before the building was erected, the first course being in 1847, by the Rev. Dr. Scoresby, of England, on the construction and use of the large telescope of Earl Rosse, and have been continued every winter up to the present time. Until within the last four years they were well attended, and no doubt produced a beneficial effect; but since the commencement of the war and the introduction into the city of a large number of sources of amusement, the audience has fallen off, or has been composed in a large degree of persons seeking amusement rather than information. The most important result produced by the lectures is that derived from their publication.

Nothing definite can be said at present as to the financial arrangements for the repair of the building. The subject is still before Congress, and although the idea has been confidently entertained that an appropriation would be made for the purpose, yet from the discussion which took place in the meeting of the joint committee of the two Houses appointed to consider this matter, I do not think a resolution authorizing such an appropriation will be adopted. In view of the impression produced by this discussion, at which I was invited to be present, I suggested to the committee that if the members would agree to recommend an appropriation to pay the back premium on coin for the last four years' interest on the Smithsonian fund, and in the event of the success of the recommendation, I thought the Regents would have it in their power to finish the repairs by means of the extra fund which has been accumulated.

Respectfully submitted.

JOSEPH HENRY, *Secretary.*

WASHINGTON, 1865.

DRAPER'S TELESCOPE.

BY THE REV. T. W. WEBB, A.M., F.R.A.S.

From the "Intellectual Observer," London.

It is gratifying to observe that, amidst all the calamities and distresses and confusion of a most unhappy civil war, the studies of peace have not been wholly lost to sight. A remarkable instance of this is afforded by the recent appearance, among the publications of the American Smithsonian Institution, of a very interesting and valuable memoir, "On the Construction of a Silvered Glass Telescope, 15½ inches in aperture, and its use in celestial photography, by Henry Draper, M.D., Professor of Natural Science in the University of New York." A copy of this, through the courtesy of the author, being now in my hands, I have thought that some account of its contents might prove interesting, especially at a time when silvered glass specula are attracting some attention in England, and (unless we are much mistaken) are likely to be more generally known and valued as most important aids to the progress of observation.

The opening sentence of this memoir requires, however, we venture to think, a little qualification. "The construction of a reflecting telescope capable of showing every celestial object now known," Dr. Draper tells us, "is not a very difficult task." We should have no hesitation in expunging the negative here, unless it were permitted to add, "when study, and labor, and ingenuity, and perseverance have been brought to bear upon it, equal to those displayed by Dr. Draper." His subsequent remark is of more universal application: "The cost of materials is but trifling compared with the result obtained; and I can see no reason why silvered glass instruments should not come into general use among amateurs. The future hopes of astronomy lie in the multitude of observers, and in the concentration of the action of many minds." His first idea was derived from an examination, in 1857, of Lord Rosse's great reflector, and of the machinery by which it was perfected; and on his return home in the following year, he resolved to construct a similar, though smaller instrument, larger, however, than any in America, and adapted to celestial photography. A metal speculum was first completed, but was split in two during the winter of 1860 by the expansion of a few drops of water that became frozen in the supporting case; and his attention was then, at Sir John Herschel's suggestion, turned to silvered glass mirrors, as reflecting more than 90 per cent. of incident light, with only ⅛th of the weight of metal. The year 1861 was occupied in overcoming the difficulties of grinding and polishing three 15½-inch disks of glass, as well as a variety of smaller pieces. Three similar mirrors were found almost essential, as two would often be so much alike that a third was necessary to gain a further step in advance. One was made to acquire a parabolic figure, (see Intellectual Observer, iii, 213,) and bore a power of 1000. The winter was spent in perfecting the art of silvering and studying photographic

processes. A large portion of 1862 was spent with a regiment in a campaign in Virginia, but in the autumn sand-clocks and clepsydras of various kinds were made, and the driving mechanism attained great excellence. During the winter the art of communicating the parabolic figure by Foucault's method was acquired, and two 15½-inch mirrors, and two of 9 inches; for enlarging photographs, were completed. The greater part of 1863 was spent in lunar and planetary photography and the enlargement of negatives, some of which were magnified to three feet in diameter. Two specula of 15½ inches were also completed, ground to an oblique focus for front view. "This work," he adds—and any one with very little experience may judge of the immense amount of toil involved—"has all been accomplished in the intervals of professional labor." Many of the expedients adopted in the working, which are detailed at full length, are strikingly characteristic of ingenuity as well as perseverance. To avoid the tediousness of grinding out defects in a metal surface, they were "stopped out," after the manner of engravers, and the uncovered space corroded away by the action of nitro-hydrochloric acid. By a similar mode, the strength of the acid being graduated in separate zones towards the edge, an increase of 15 inches in focal length was gained. The grinder and mirror were at another time included in a voltaic circuit to abridge the grinding process, and an idea was entertained of saving much weight by electrotyping a brass mirror with speculum metal. When he commenced operations with glass he had to polish with his own hands more than one hundred mirrors of various sizes, from 19 inches to ¼ inch, and to experience very frequent failures for three years before he was able to produce large surfaces certainly and speedily. His labor would have been much diminished, inasmuch as he would have been spared the causeless condemnation of many fine mirrors, as well as the working of some *square* ones, had he become earlier aware of an important fact respecting the rigidity of the material.* Generally speaking, a sheet of glass, even when very thick, can hardly be set on edge without so much flexure as to render it optically worthless; but, fortunately, in every disk that he tried, there was one diameter on either end of which it might stand without harm. On turning a disk of 15½ inches, with a thickness of 1¼ inch, one quarter round, it could hardly be realized that the surface was the same: 90° more restored it to its original defining power; and this effect was found to be independent of any irregularity on the edge of the disk and of the mode of support. Dr. Draper refers it, with great probability, to the structure of the glass, resulting from its having been subjected to rolling pressure. A similar irregularity of structure is known to obtain in many large object-glasses, and Dr. Draper specifies the great achromatic by Cauchoix, presented by the late Duke of Northumberland to the University of Cambridge, as having had its lenses turned round by Mr. Airy in mounting, for this reason. Short's Gregorian specula, too, were always marked on the same account. The strange deformations of image produced by heat, even by

* In examining and testing last year some fine 8-inch specula of Mr. With's workmanship, I had independently ascertained this peculiarity, so far as a *best position* for each was concerned, but I stopped short of Dr. Draper's discovery of a regular *axis of rigidity*.

the warmth of the hand for a few seconds, are described and represented. From such distortions the speculum would not recover in ten minutes, and the error would be rendered permanent by repolishing in that condition; and so injurious may such causes, even in a lesser degree, prove during the delicate process of the final correction of the spherical error, that "a current of cold or warm air, a gleam of sunlight, the close approach of some person, an unguarded touch, the application of cold water injudiciously, will ruin the labor of days." He found it a matter of not unfrequent occurrence that a speculum would perform much better with rays of a certain amount of obliquity,* deviating from 2° to 3° from the axis. It is obvious that if this peculiar form could be produced at will, and to an adequate degree, it would render the Lemairean or front view telescope perfect. Dr. D., however, found that the image was never quite as fine as in the usual kind of mirrors. A letter of Maskelyne subsequently came under his notice, in which he describes a very great improvement effected in a 6 foot reflector by Short, by inclining the large speculum 2½°, and remarks, very reasonably, that "probably it will be found that this circumstance is by no means peculiar to this telescope;" a hint which may be worthy of the consideration of the possessors of reflectors. Such surfaces require to be reground, or "re-fined," *i. e.*, finished with the finest emery, to get rid of this obliquity, as repolishing, though occasionally successful in a few minutes, will not always effect it; the attempt failed in one instance, though continued for 13½ hours.

The modes of forming the requisite tools, of preparing emery, of grinding, polishing, testing, (by Foucault's mode,) and silvering the surface, are somewhat too technical to find a place here, but some interesting facts are worthy of being referred to. Such is the effect produced by the removal from a cast-iron tool, 15½ inches in diameter, divided into ¾-inch squares, like a chessboard, of every alternate square, by an acid. Though the corrosion extended only to a very slight depth, it flattened the curvature of the tool 7¾ inches. "This shows what a state of tension and compression there must be in such a mass, when the removal of a film of metal $\frac{1}{50}$th of an inch thick, here and there, from one surface, causes so great a change." Another important remark is, how injurious an atmospheric disturbance is set up by the intermixture of currents of warm air from the observer's person with the rays falling on or reflected from the mirror—an observation which I made many years ago, and which any one may test by directing a Newtonian to any bright object, and placing one hand beneath the aperture, while an eye-piece held in the other hand, and applied to the eye, is carried back a considerable distance, so as to obtain a very long focus, and render the ascending currents more visible. It has not, I believe, been generally remarked how prejudicial an effect this must have on definition in the front-view reflector, and it would be a worthy object of attention to remove the evil by the interposition of some non-conducting shield.

* I became acquainted with this fact many years ago, when working metals for a small Newtonian reflector.

A full trial was given to no less than seven machines, on the principles employed by Lord Rosse and Lassell, with modifications of his own. The prime mover, called the "foot-power," was a very ingenious contrivance, in which very little force is lost in overcoming friction, and which is frequently employed in America for dairy use. Dr. D. himself generally walked in his own, and has travelled some days, during five hours, more than ten miles. It consists of an endless band of short transverse boards or "treads," interlocking so as to form a platform to tread upon, which will not yield downwards on its upper side, but hangs loose in the return half beneath, and passing over wheels and rollers at either end. This succession of boards, having one end a little higher than the other, runs downwards as soon as a weight is placed upon it, and communicates motion to a large wheel on the axle of the one over which it turns, and through it to any connected machinery. Being placed between a handrail on either side, it offers the appearance of a little narrow bridge, as over a ditch, composed of transverse boards, on which the mover may walk all day without getting a step forward. It is, in fact, a species of treadmill, of a much more pleasant construction.

The mode of giving a parabolic figure finally preferred by Dr. D. is that of "local retouches," in which the edge of a spherical mirror is flattened, or, which he thinks preferable, the centre is bored out deeper, by appropriate polishers of curvatures differing slightly from that of the original tool on which it was wrought. This method, as invented by M. Léon Foucault, at Paris, was employed by hand, but has been practiced by Dr. D. with suitable machinery, and with excellent results; his great specula, thus finished, bearing a power of 1200, and dividing the celebrated test-pair γ^2 *Andromedæ;* while so great is the light-collecting power of 15½ inches, that the companion of *Wega* can be perceived even with the unsilvered surface; some portions of the moon are even more visible than after silvering—a hint worth notice. When silvered, the quantity of lunar light is so overpowering as to impair for a long time the vision of an eye placed at the focus. Several modes of silvering were tried by Dr. D., some devised by himself. Foucault's proved uncertain in its results; that of Cimeg, with tartrate of potash and soda, for looking-glasses, modified so as to fit the silver for being polished on the reverse side, he found superior to any, and in using it "never on any occasion failed to secure bright, hard, and in every respect perfect films." Their thickness is about $\frac{1}{200000}$ of an inch—nearly the same with that of gold-leaf of equal transparency—the sun appearing through the silver of a light-blue tint. Variations in its thickness are consequently only small fractions of that fraction, and of no optical moment whatever. It tarnishes quickly if exposed to sulphuretted hydrogen—a defect which has been avoided in the English process—and it may be split up into fissures by damp; but heat does not affect it, and it is generally very enduring. "I have some," the doctor says, "which have been used as diagonal reflectors in the Newtonian, and have been exposed during a large part of the day to the heat of the sun concentrated by the 15½-inch mirror. These small mirrors are never covered, and yet the one now in the telescope has been there a year, and has

had the dusty film, like that which accumulates on glass, polished off it a dozen times."

Besides other interesting optical particulars, the memoir contains many directions for the successful practice of celestial photography, some of which might be found equally valuable for terrestrial purposes; and to these we may advert on a future occasion, adopting for the present the author's closing remarks: "In concluding this account of a silvered glass telescope, I may answer an inquiry which, doubtless, will be made by many of my readers, whether this kind of reflector can ever rival in size and efficiency such great metallic specula as those of Sir W. Herschel, the Earl of Rosse, and Mr. Lassell? My experience in the matter, strengthened by the recent successful attempt of M. Foucault to figure such a surface more than thirty inches in diameter, assures me that not only can the four and six feet telescopes of those astronomers be equalled, but even excelled. It is merely an affair of expense and patience. I hope that the minute details I have given in this paper may lead some one to make the effort."

BRIEF REVIEW OF A MEMOIR ON THE CRETACEOUS REPTILES OF THE UNITED STATES, PUBLISHED IN THE FOURTEENTH VOLUME OF THE SMITHSONIAN CONTRIBUTIONS TO KNOWLEDGE.

BY THE AUTHOR, JOSEPH LEIDY, M. D

[illegible] formations are so named from the Latin *creta*, chalk, from the [illegible] England forms the most striking rock of the series. [illegible] immediately precede the tertiary formations, and contain a [illegible] Plants are rare because the cretaceous rocks are all of [illegible] consist of deposits which were formed at the bottom of the [illegible] animals are numerous, but they are confined to the [illegible] authentic traces of birds or mammals have yet been dis-[illegible]

[illegible] as indicated by their contained fossils and relative [illegible] are widely extended throughout the United States, [illegible] true chalk. They are mainly composed of beds of [illegible] compact limestones. Among the sandy layers are extensive [illegible] green sand, which, under the name of marl, is much em-[illegible] fertilizing material.

The cretaceous formations extend in a large tract through New Jersey, Maryland, and Delaware, and appear in isolated patches in North and South Carolina and Georgia. From the western part of the latter State they curve in a wide crescent-like tract through Alabama, Mississippi, and Tennessee, to [illegible] of the Ohio river. Thence passing in a narrow band through [illegible] they expand so as to occupy a great portion of the region between [illegible]

the Mississippi river and the Rocky mountains, reaching north into British America, and south into Mexico. In New Jersey they are estimated to have a thickness of from four to five hundred feet; in the region of the Upper Missouri, from two to two thousand five hundred feet.

Multitudes of fossils are found in the American cretaceous formations, though the species appear not to be so numerous as in those of Europe. The mollusks are particularly abundant, and among them are a great many species of chambered shells. A species of ammonite is found on the Upper Missouri as large as an ordinary fore-wheel of a wagon. Remains of fishes are likewise numerous, but generally they are found in a very fragmentary condition. Scales, vertebræ, and teeth, are usually the fossils which represent them. The teeth of sharks are especially numerous. Bones of reptiles are also abundant, and their remains form the subject of the author's memoir. As in the European cretaceous formations, no evidences have yet been discovered of the existence of birds or mammals in those of the United States.

Most of the reptilian remains described by the author have been derived from New Jersey, where they are constantly being discovered in the digging of marl for agricultural purposes. Various genera and species of the crocodile family existed during the cretaceous period, as indicated by their remains. Some of these had the skeleton constructed after the same pattern as those now living, while others were peculiar, or have no near representatives in existence.

All living crocodiles, under which term we include the gavials and alligators, have the vertebræ or bones of the spinal column, with their bodies concave in front and convex behind, so that they are firmly jointed in a ball and socket-like manner. Several species of extinct crocodiles, having the spinal column constructed in the same manner, have been discovered in the cretaceous limestone and green-sand of New Jersey and Delaware. One of the latter species, named *Thoracosaurus neocesariensis*, signifying the armor-covered saurian of New Jersey, resembled in its form and size the modern long-snouted gavial, or crocodile of the Ganges. A nearly entire skull of this animal was discovered imbedded in limestone on the farm of General William Irick, near Vincenttown, Burlington county, New Jersey, and is now preserved in the museum of the Academy of Natural Sciences of Philadelphia. Fragments of jaws and long, curved, conical teeth of the same species have been found in other localities of the State, as the highlands of Neversink, the vicinity of Blackwoodtown, Camden county, Big Timber creek, Gloucester county, and in Burlington county. Vertebræ and other bones, including specimens of the strong osseous plates of the skin, have likewise been discovered in Burlington county.

The skull of this crocodile, when perfect, has measured over a yard in length, and the whole animal about twenty-five feet.

A species of the same genus, the *Thoracosaurus macrorhynchus* has been discovered in a cretaceous formation of France, and a fine skull of it is preserved in the museum of the Jardin des Plantes, at Paris.

Another extinct crocodile, named *Bottosaurus Harlani*, after Dr. Harlan, who

first noticed the species in 1824, is yet only known from several fragments of jaws, a few teeth, and vertebræ, found in the green-sand of Burlington county, New Jersey. The specimens indicate this crocodile to have resembled in its construction and size the alligator of the Mississippi, or the crocodile of the Nile, more nearly than the Gangetic gavial. Several smaller species of crocodiles, with concavo-convex vertebræ, or vertebræ of the same construction of those of living crocodiles, are indicated by specimens of vertebræ and other bones found in the green-sand of New Jersey and Delaware.

Another crocodilian reptile, the remains of which have been found in the green-sand of New Jersey, is the *Hyposaurus Rogersii*, named by Professor Owen, of London, after Professor Rogers, who submitted several vertebræ of the animal to his inspection about fifteen years ago. This crocodile belongs to a more ancient type of structure than the former ones, and has no near living representative. The vertebræ have their bodies dished at both extremities, or biconcave, as in fishes, though in a much less degree. No considerable portion of the skeleton of this species has yet been discovered, and its remains are usually in an exceedingly friable condition. The author has had the opportunity of observing specimens of vertebræ, fragments of bones of the limbs, and teeth of about eight different individuals. The teeth are long, narrow. and curved conical like those of the Gangetic gavial, but are more or less compressed, so as to present anterior and posterior trenchant or cutting borders, The animal did not exceed in size the alligator of the Mississippi.

Another crocodilian reptile, much larger than any of the preceding, and constructed after a different type, has been named *Discosaurus vetustus*. Only vertebræ and some of the small bones of the limbs of the species have as yet been found. The bodies of the vertebræ have their articular ends flat, or nearly so, and they bear a general resemblance with the corresponding portions of the vertebræ of the living cetaceous, or animals of the dolphin and whale order. The genus is named from the articular ends of the vertebral bodies appearing as distinct disks or plates implanted on the latter. Remains of six different individuals have been observed by the author from the green-sand of New Jersey and from other cretaceous formations of Georgia, Alabama, and Mississippi. A few bones of a foot of this reptile from New Jersey indicate the limbs to have been constructed as fins, so that the animal was more eminently aquatic than the true crocodiles.

An equally large crocodilian reptile with the one last indicated, and closely allied to it, as proved by the construction of the vertebræ, has been named *Cimoliasaurus magnus*, the generic term signifying its contemporaneous age with the chalk. The remains of this animal, consisting of a number of vertebræ, have as yet only been discovered in the green-sand of Burlington and Monmouth counties, New Jersey.

To conclude with the American cretaceous crocodiles, a large tooth, belonging to the museum of the Smithsonian Institution, from a deposit of the Red siver of the North, indicates a peculiar species, to which the name of *Piratosaurus plicatus* has been given. The specimen was found in association with a number of shark teeth, of species evidently of the cretaceous epoch.

One of the most extraordinary reptiles which existed during the cretaceous period, both in Europe and America, is the *Mosasaurus*, or lizard of the Meuse. It was of gigantic size, and is most nearly related, among living reptiles, to the comparatively puny lacertians and monitors.

A nearly entire skull, including the jaws and teeth, of the *Mosasaurus camperi*, was discovered in 1780 in one of the subterraneous quarries of St. Peter's Mount at Maestricht. When found, the quarrymen gave notice of its discovery to Dr. Hoffman, a surgeon of Maestricht, who collected fossils. Dr. Hoffman succeeded in safely removing the skull from its position in the quarry, imbedded in a large block of stone, and had it conveyed to his residence. The remarkable specimen having attracted much attention, its fame reached the ears of a reverend canon who owned the ground above the quarry from whence the skull had been obtained. The canon laid claim to the specimen and applied to law for its possession. After a troublesome suit he obtained it, much to the cost and chagrin of Dr. Hoffman. In 1795 the army of the French republic laid siege to Maestricht and bombarded Fort St. Peter, near which was the country residence of the canon, where the fossil skull was preserved. The general of the French having been informed of the circumstances relating to the fossil, gave orders that the artillerists should avoid that particular quarter. The canon suspecting the object of this exemption, had the skull conveyed to a place of safety in the city. After the army obtained possession of the latter, Freicine, the representative of the people, promised a reward of six hundred bottles of wine for the recovery of the skull, which had the desired effect, for the following day a dozen grenadiers bore the specimen in triumph to his house. It was subsequently conveyed to Paris, and now forms part of the collection of the museum of the Jardin des Plantes. The skull of the Maestricht Mosasaurus, or Maestricht monitor, as it is also called, was nearly four feet long; the lower jaw three feet and three-quarters. The jaws were occupied by fifty-six teeth, besides which there were sixteen at the entrance of the throat on the ptemygoid bones. The teeth are large; have curved conical crowns, with the surfaces subdivided into narrow planes; and have remarkably robust fangs inserted into deep sockets of the jaws, with which they became firmly co-ossified.

Cuvier estimated the number of vertebræ to have been one hundred and thirty-three, and their bodies are concavo-convex, as in living crocodiles. The tail vertebræ are especially remarkable from their construction, being provided, as in fishes, with a co-ossified arch and spine below the bodies as well as above. The character of these caudal vertebræ indicates the tail to have been laterally flattened and of great comparative depth, and thus well adapted to the aquatic habits of the animal.

Remains of a species of *Mosasaurus*, equally huge with the Maestricht monitor, are frequently found in New Jersey in the digging of green-sand for agricultural purposes, but they are usually in a very fragmentary condition. Nevertheless, not a year has passed within the last thirty that isolated teeth, vertebral fragments of jaws, and other bones, have not been turned up in the

diggings of the marl. Some of the specimens of teeth exceed six inches in length, but generally they are smaller.

The teeth present considerable variety, generally having curved conical or pyramidal crowns, frequently more or less compressed, with the surfaces in different degrees subdivided into narrow planes or nearly or quite devoid of them. The root or fang is cylindrical and several times the bulk of the crown, and is inserted in a deep socket with which it is usually observed firmly co-ossified, though it may also be loose, depending on the age or state of development of the tooth. New teeth were incessantly developed as those in use were worn away. They made their appearance at the back part internally of the fangs of the latter, and their fangs were gradully hollowed to accommodate the growth of the new teeth. The crowns of the old ones were then shed to allow the new ones to occupy their place, and after the full growth of the new teeth their fangs co-ossified with their containing sockets. A constant succession of teeth in this manner took place during the life of the *Mosasaurus*.

The remains of the New Jersey *Mosasaurus* have been referred to a species named *M. Mitchilli*, in honor of Dr. S. L. Mitchill, of New York, who first directed attention to their existence in this country in 1818. A few fossils, apparently of the same species, have been found in North and South Carolina. Remains of *Mosasaurus*, probably of a different species from the former, have been discovered on the Upper Missouri, of which a notice was first given by Dr. R. Harlan, in 1834. The greater part of a skeleton was subsequently found by Major O'Fallon, imbedded in a rock in the vicinity of Big Bend, and was presented by him to Maximillian, Prince of Wied, who was then travelling in western America. The prince had the specimen conveyed to Europe and presented to the Academy of Naturalists at Bonn, in the museum of which it is now preserved. Dr. Goldfuss, who described the specimen, estimated the number of vertebræ to have been one hundred and fifty-seven. We have but little certain knowledge of the bones of the limbs of *Mosasaurus;* but the more authentic specimens which have been found go to show that the animal was provided with fins adapted to swimming.

Remains apparently of a comparatively small species of *Mosasaurus*, or of a closely allied genus, have been discovered in the cretaceous formations of Alabama and Mississippi.

Some isolated vertebræ of large size, from the green-sand of New Jersey, are supposed by Professor Owen, of London, to indicate a saurian distinct from *Mosasaurus*, though allied to it, to which he has given the name of *Macrosaurus lævis*. Similar vertebræ have been found in North Carolina. A long, narrow, conical tooth crown, with the surfaces subdivided into planes, from the green-sand of Burlington county, New Jersey, and an exactly similar one from the shores of Cape Fear river, North Carolina, indicate another reptile, probably allied to *Mosasaurus*, which have been named *Polygonodon vetus*.

Perhaps the most extraordinary reptile yet discovered in the American cretaceous formations is an enormous herbivorous lizard, the *Hadrosaurus Foulkii*. A gentleman of Philadelphia, W. Parker Foulke, while passing the warm season

of 1858 in the pleasant little village of Haddonfield, Camden county, New Jersey, was informed by a neighbor, Mr. J. E. Hopkins, that some remarkable bones of huge size had been discovered while digging marl upon his farm about twenty years previously. The specimens first found had been all given away or lost. Under the expectation of finding others, Mr. Foulke employed men to dig in the position of the old excavation, which was in a ravine through which flowed a branch of Cooper's creek. At the depth of nine feet numerous bones were found in a bed of tenacious blue clay, mingled with a multitude of fossil shells. The bones, though fractured, were otherwise well preserved, and exhibited no appearance of being water-rolled. Indeed, the most delicate of the accompanying shells, though decomposed, had preserved their forms so perfectly that it was evident the animal remains had originally rested on the soft mud at the bottom of a quiet sea. The bones which were obtained consisted of twenty-eight vertebræ, part of the pelvis, most of the bones of the left fore and hinder extremities, some small fragments of jaws, and nine teeth.

The vertebræ of the neck and forepart of the dorsal region have their bodies convex in front and concave behind, the reverse of the condition in the living crocodiles, and in the extinct Mosasaurus. The degree of convexity and concavity declines in the posterior dorsal vertebræ, and in the vertebræ of the loins and tail the bodies are biconcave. The anterior caudal vertebræ are the largest of the spinal column, though not so long as most of the others. A perfect specimen indicates the tail to have been of enormous size—near the root about a foot and a half in vertical diameter, and eight inches transversely. The humerus or arm-bone is twenty-two and a half inches long, and nearly seven inches broad at the upper part. The bones of the forearm have about the same length. The bones of the hinder extremity are especially remarkable for their huge proportions, whether viewed independently or in relation with those of the fore extremity. The femur, or thigh-bone, is forty-one inches and a half long and fifteen inches in circumference near the middle. The tibia, or shin-bone, is over three feet long, and nearly a foot in circumference about the middle. Both the humerus and femur contain large medullary cavities.

The remains of *Hadrosaurus* exhibit a close relationship of the reptile with the *Iguanodon*, a lizard of equally huge proportions and like habits, discovered by Dr. Mantell in the next oldest formation to the cretaceous, known as the wealden of Europe. The specimens of the *Iguanodon Mantelli* now form part of the magnificent collection of the British Museum.

Hadrosaurus and *Iguanodon*, in proportions and habits, held the same relationship with other great extinct lizards that the bulky herbivorous pachyderms do among ordinary mammals. They might be viewed as the oxen among the tigers, and insect eaters of lizards.

Among living lizards, the Iguanas of South America, and the marine Amblyrhynchus of the Galipagos islands, are the only ones which are vegetable feeders, and in these the sharp, serrated teeth are only adapted to cutting the softer kinds, such as fruits, flowers, sea-weed, &c., and are not at all fit for mastication or grinding the food.

When Dr. Mantell first exhibited teeth of the Iguanodon, no one would believe that they belonged to a reptile, and even Cuvier pronounced a worn specimen to be the tooth of a rhinoceros.

The teeth of *Hadrosaurus* have the same general constitution as those of *Iguanodon*, being adapted to the trituration of vegetable food. They are exceedingly small in relation with the size of the animal, measuring only a little over an inch in length, but they were numerous, and appear to have been arranged so closely together as to form a continuous pavement at the border of the jaws, well adapted to the crushing and comminution of vegetable substances. As the teeth in use were worn away, they were incessantly followed by others, which also appear to have been arranged in close apposition with one another.

Hadrosaurus was probably an amphibious reptile. Its huge, laterally flattened tail was evidently adapted to swimming. The large hollows in the interior of the arms and thigh bones would indicate a partially terrestrial habit. The great disproportion between the fore and back parts of the body has led to the view that when not swimming the huge reptile supported itself in a frog-like position, though it had an additional prop in the huge tail.

Teeth nearly like those of *Hadrosaurus*, but referred to another reptile, named *Trachodon*, have been discovered by Dr. F. V. Hayden, in a formation of unascertained age, though probably cretaceous, in the bad lands of the Judith river, a tributary of the Missouri, near its source.

Other huge bones of reptiles have been discovered in the New Jersey green-sand, of uncertain reference, but most probably of species allied to *Hadrosaurus*. Several bones, from Burlington county, of a reptile of comparatively small size with the latter, though also probably allied to it, are remarkable for their hollowness, almost approaching in this respect the condition of the bones of birds. They have been referred to a genus under the name of *Cœlosaurus*, the term referring to the hollowness of the skeleton.

Several teeth of a large reptile, discovered in a cretaceous formation near Bladensburg, Maryland, have been referred by Dr. Johnson, of Baltimore, to a peculiar genus, under the name of Astrodon.

A large tooth, mingled with a number of others of sharks, from the green-sand of Mullica Hill, Lancaster county, New Jersey, indicates a carnivorous reptile, to which the name of *Tomodon horrificus* is applied. The tooth resembles a large shark tooth, being broad, flattened conical, with sharp, cutting borders minutely serrated. The original possessor of the tooth was no doubt a fierce and sanguinary cotemporary of the peaceful, herb-eating *Hadrosaurus*.

Of turtles, the green-sand of New Jersey has furnished the fossil remains of a number of genera and species. The fossils are, however, generally in a very imperfect state, usually consisting of fragments of a few plates of the bony shell. The remains observed by the author appear to indicate eight species of five genera. Of these, two are referred to *Chelone*, three to *Emys*, one to *Platemys*, one to *Trionyx*, and one to a peculiar genus—*Bothremys*. The latter is remarkable from the jaws being provided with large funnel shaped pits, one

on each side, those of the upper jaw being opposed to the lower ones. It is difficult to conjecture the use of these pits, though the author has suspected that probably these sprung from the tooth-like processes of the horny beaks with which the jaws of turtles are furnished. A skull, the only part yet discovered which is distinctly referrible to the genus, was obtained from the green-sand near Barnsboro', Gloucester county, New Jersey. It bears a resemblance, among living turtles, most nearly to the great Amazon turtle, Podocnemys. The species is named *Bothremys Cookii*, in honor of Professor George H. Cook, of Rutger's college, New Brunswick, New Jersey, who greatly assisted the author in procuring specimens for his examination, which form part of the material of the memoir.

APPENDIX TO THE REPORT OF THE SECRETARY.

REPORT OF THE ASSISTANT SECRETARY.

SMITHSONIAN INSTITUTION,
Washington, D. C., December 31, 1864.

SIR: I beg herewith to present a report, for 1864, of the operations which you have intrusted especially to my charge; mainly, those relating to exchanges and the collections.

Very respectfully, your obedient servant,

SPENCER F. BAIRD,
Assistant Secretary Smithsonian Institution.

Prof. JOSEPH HENRY, LL.D.,
Secretary Smithsonian Institution.

EXCHANGES AND TRANSPORTATION.

The distribution of publications of the year included volume xiii of the Smithsonian Contributions to Knowledge, of 558 pages and seven plates; volume v of Miscellaneous Collections, of 774 pages; and the Annual Report of the Institution for 1862, of 450 pages; making an aggregate of 1,782 pages and seven plates.

As heretofore, the packages transmitted and received included all the exchanges between the learned institutions and men of science generally of America and other parts of the world, the details of which are shown in the accompanying tables. As in previous years, the Institution has to make its grateful acknowledgments for services rendered in connexion with these operations by the Cunard, the Bremen, the Hamburg, and the Pacific Mail Steamship lines; the Panama Railroad Company, the Adams Express Company, the Hudson's Bay Company, &c. Privileges similar to those granted heretofore by the Adams Express Company have, during the year, been also extended by the Harnden and the American Express Companies.

To Messrs. Hiram Barney and Simeon Draper, collectors of the port of New York, and Mr. George Hillier, of the custom-house; and to Mr. Samuel Hubbard, of the Pacific Mail Steamship Company, in San Francisco, the Institution continues to be under many obligations for important aid extended in connexion with its system of exchanges and transportation. Its thanks are also due to Messrs. F. Probst & Co., of New York, and their correspondents in Vera Cruz, Messrs. Leffmann and Gutheil, for valuable assistance in the exchanges with Mexico.

The regular foreign agents of the Institution—Dr. Felix Flügel, of Leipsic; Messrs. Gustave Bossange & Co., of Paris; Mr. William Wesley, of London; and Mr. Frederick Müller, of Amsterdam—have continued to discharge their duties to the full satisfaction of the Institution.

A.

Receipts of books, &c., by exchange in 1864.

Volumes:		
Octavo	645	
Quarto	153	
Folio	25	
		823
Parts of volumes and pamphlets:		
Octavo	1,945	
Quarto	680	
Folio	129	
		2,754
Maps and charts		109
Total		3,686

B.

Table showing the statistics of exchanges of the Smithsonian Institution in 1864.

Agent and country.	Number of addresses.	Number of packages.	Number of boxes.	Bulk of boxes in cubic feet.	Weight of boxes.
DR. F. FLÜGEL, *Leipsic*—					
Sweden	12	13			
Norway	8	12			
Russia	48	51			
Germany	306	357			
Switzerland	36	36			
Belgium	17	18			
			25	207	8,820
FRED. MÜLLER, *Amsterdam*—					
Denmark	10	12			
Holland	38	40			
			3	27	780
GUSTAVE BOSSANGE & CO., *Paris*—					
France	93	104			
Italy	50	53			
Spain and Portugal	11	12			
			12	100	4,000
W. WESLEY, *London*—					
Great Britain and Ireland	149	203			
			13	112	4,300
Rest of the world	65	100	10	100	2,600
Total	843	1,011	63	546	20,500

C.

Addressed packages received by the Smithsonian Institution from parties in America, for foreign distribution, in 1863.

	Parcels.
ALBANY, NEW YORK.	
New York State Library	6
AMHERST, MASSACHUSETTS.	
Dr. C. H. Hitchcock	10
BOSTON, MASSACHUSETTS.	
American Academy of Arts and Sciences	140
Society of Natural History	212
CAMBRIDGE, MASSACHUSETTS.	
Library of Harvard College	19
Museum of Comparative Zoölogy	550
Nautical Almanac	43
Alexander Agassiz	63
A. S. Packard	12
Prof. H. J. Clark	24
COLUMBUS, OHIO.	
Ohio State Agricultural Society	80
DORCHESTER, MASSACHUSETTS.	
Dr. Edward Jarvis	13
INDIANAPOLIS, INDIANA.	
Institution for the Deaf and Dumb	12
IOWA CITY, IOWA.	
Prof. G. A. Hinrichs	17
JACKSONVILLE, ILLINOIS.	
Illinois State Hospital for Insane	60
JANESVILLE, WISCONSIN.	
Institution for the Blind	41
MONTREAL, CANADA.	
Prof. J. W. Dawson	26
NEW HAVEN, CONNECTICUT.	
American Journal of Science	30
American Oriental Society	5
NEW YORK.	
New York Lyceum of Natural History	82
Thomas Bland	20
Dr. Henry Draper	37
PHILADELPHIA, PENNSYLVANIA.	
Academy of Natural Sciences	173
Entomological Society of Philadelphia	27
American Pharmaceutical Association	45
George W. Tryon, jr	2
Dr. S. W. Mitchell	35
Dr. Wilcox	25
PROVIDENCE, RHODE ISLAND.	
State of Rhode Island	9
ST. LOUIS, MISSOURI.	
Academy of Sciences	8
SANTA BARBARA, CALIFORNIA.	
A. S. Taylor	4
TORONTO, CANADA.	
Canadian Institute	4
Observatory	24
WASHINGTON, D. C.	
United States National Observatory	135
United States Patent Office	781
United States Coast Survey	269
United States Treasury Department	369
Department of Agriculture	50
Total	3,462

D.

Addressed packages received by the Smithsonian Institution from Europe, for distribution in America, in 1864.

	No. of packages.		No. of packages.
ALBANY, NEW YORK.		**BOSTON, MASS.—Continued.**	
Albany Institute	2	Perkins Institute and U. S. Asylum for Blind	2
Dudley Observatory	11	Prison Discipline Society	1
New York State Agricultural Society	29	Public Library	13
University of the State of New York	4	State Library	14
State Library	41	Dr. Charles Beck	2
Prof. J. Hall	8	Alvan Clarke	1
Franklin B. Hough	1	Dr. John Dean	1
AMHERST, MASSACHUSETTS.		Dr. A. A. Gould	4
		Colonel J. D. Graham	14
Amherst College	5	Rev. Mr. Grout	1
Dr. E. Hitchcock	3	John R. Motley	3
Prof. C. U. Shephard	1	Prof. W. B. Rogers	1
		Charles Sprague	1
ANNAPOLIS, MARYLAND.		Dr. D. H. Storer	1
		Hon. Charles Sumner	1
State Library of Maryland	5	George Ticknor	2
		Hon. R. C. Winthrop	1
ANN ARBOR, MICHIGAN.			
		BRATTLEBORO', VERMONT.	
Observatory	5		
University of Michigan	1	Vermont State Lunatic Asylum	1
Dr. Brunnow	4		
		BROOKLYN, NEW YORK.	
AUGUSTA, MAINE.			
		J. C. Brevoort	1
State Library	4		
State Lunatic Hospital	1	**BRUNSWICK, MAINE.**	
AUSTIN, TEXAS.		Bowdoin College	3
		Prof. P. A. Chadbourne	1
State Library	1	A. S. Packard, jr	3
BALTIMORE, MARYLAND.		**BUFFALO, NEW YORK.**	
Maryland Historical Society	3	F. Husted	1
Dr. John G. Morris	2		
		BURLINGTON, IOWA.	
BATON ROUGE, LOUISIANA.			
State Library	3	Iowa Historical and Genealogical Institute	1
BOSTON, MASSACHUSETTS.		**BURLINGTON, VERMONT.**	
American Academy of Arts and Sciences	123	University of Vermont	2
American Board of Commissioners for Foreign Missions	1	**CAMBRIDGE, MASSACHUSETTS.**	
Atlantic Monthly	1	American Association for the Advancement of Science	32
Boston Athenæum	2	Astronomical Journal	2
Boston Society of Natural History	145	Harvard College	25
Bowditch Library	2	Museum of Comparative Zoölogy	4
Geological Survey of Massachusetts	1	Observatory of Harvard College	25
Massachusetts Historical Society	3	Cleveland Abbe	2
North American Review	2	Prof. L. Agassiz	54
New England Historic-Genealogical Society	3	Prof. G. P. Bond	5

D.—*Addressed packages received by the Smithsonian Institution, &c.*—Continued.

	No. of packages.
CAMBRIDGE, MASS.—Continued.	
Prof. A. Agassiz	1
Prof. H. J. Clark	3
Miss Felton	1
Prof. Wolcott Gibbs	1
Prof. Goodwin	1
Dr. B. A. Gould	10
Dr. Asa Gray	28
Prof. Henry W. Longfellow	1
Prof. Jules Marcou	7
Charles E. Norton	1
Prof. B. Peirce	13
F. W. Putnam	1
T. H. Safford	2
J. Langdon Sibley	1
Jared Sparks	1
Prof. J. E. Worcester	5
Prof. J. Wyman	2
CHICAGO, ILLINOIS.	
Academy of Sciences	12
Chicago Historical Society	1
Mechanics' Institute	2
Robert Kennicott	2
CINCINNATI, OHIO.	
American Medical College	1
Historical and Philosophical Society	1
Mercantile Library	3
Observatory	4
J. Taft	1
CLEVELAND, OHIO.	
Dr. J. S. Newberry	1
CLINTON, NEW YORK.	
Hamilton College	1
Hamilton College Observatory	2
Dr. C. H. F. Peters	1
COLUMBIA, MISSOURI.	
Geological Survey of Missouri	8
Missouri University	1
State Library	1
COLUMBIA, PENNSYLVANIA.	
[illegible] S. S. Haldeman	1
COLUMBUS, OHIO.	
State Board of Agriculture	72
[illegible]	6
[illegible]	2

	No. of packages.
CONCORD, NEW HAMPSHIRE.	
Natural History Society	1
New Hampshire Asylum for Insane	2
State Library	4
DES MOINES, IOWA.	
State Library	26
DETROIT, MICHIGAN.	
Michigan State Agricultural Society	21
Office of Survey of the North American Lakes	1
Dr. Tappan	1
DORCHESTER, MASSACHUSETTS.	
Hon. Edward Everett	2
Dr. Edward Jarvis	1
EASTON, PENNSYLVANIA.	
Dr. Brackenridge Clemens	1
Prof. James H. Coffin	1
ERIE, PENNSYLVANIA.	
Rev. L. Olmstead	1
FRANKFORT, KENTUCKY.	
Geological Survey of Kentucky	11
State Library	4
GAMBIER, OHIO.	
Kenyon College	10
GEORGETOWN, D. C.	
Georgetown College	7
Dr. L. Mackall	1
Dr. A. Schott	2
HARRISBURG, PENNSYLVANIA.	
Pennsylvania State Lunatic Hospital	1
State Library	8
HARTFORD, CONNECTICUT.	
Historical Society of Connecticut	2
Retreat for Insane	2
State Library	5
Young Men's Institute	1
Hon. Henry Barnard	1
HUDSON, OHIO.	
Western Reserve College	3

D.—*Addressed packages received by the Smithsonian Institution, &c.*—Continued.

	No. of packages.
INDIANAPOLIS, INDIANA.	
Indiana Hospital for Insane	1
State Library	5
IOWA CITY, IOWA.	
Iowa State University	13
Prof. Hinrichs	1
JACKSON, MISSISSIPPI.	
State Library	2
JACKSONVILLE, ILLINOIS.	
State Hospital for the Insane	2
State Institution for the Blind	1
JANESVILLE, WISCONSIN.	
State Institution for the Blind	7
JEFFERSON CITY, MISSOURI.	
State Library	3
KALAMAZOO, MICHIGAN.	
Michigan Asylum for Insane	1
LANSING, MICHIGAN.	
State Agricultural College	4
State Library	4
LEXINGTON, KENTUCKY.	
Eastern Lunatic Asylum	1
LITTLE ROCK, ARKANSAS.	
State Library	2
MADISON, WISCONSIN.	
Historical Society of Wisconsin	5
Skandinaviske Presse-Forening	1
State Library	11
Wisconsin State Agricultural Society	25
MONTPELIER, VERMONT.	
State Library	9
MONTREAL, CANADA.	
McGill University	2
Natural History Society	7
Dr. J. W. Dawson	2
Dr. W. H. Kingston	1

	No. of packages.
NANTUCKET, MASSACHUSETTS.	
Miss Maria Mitchell	1
NEW BRUNSWICK, NEW JERSEY.	
Prof. George H. Cook	8
NEWBURG, OHIO.	
Longview Asylum	1
NEW HAVEN, CONNECTICUT.	
American Journal of Science	39
American Oriental Society	13
Yale College	7
Prof. G. J. Brush	4
Prof. J. D. Dana	29
Prof. Loomis	6
E. C. Marsh	2
Prof. B. Silliman	13
Prof. W. D. Whitney	7
NEW ORLEANS, LOUISIANA.	
New Orleans Academy of Sciences	27
NEWPORT, RHODE ISLAND.	
United States Naval Academy	1
NEW YORK, N. Y.	
American Agriculturist	1
American Ethnological Society	2
American Geographical and Statistical Society	45
American Institute	3
Astor Library	8
Farmer and Mechanic	1
Historical Society	1
Mercantile Library Association	1
New York City Asylum, (Blackwell's island)	1
New York Dental Journal	2
New York Institution for Blind	1
New York Lyceum of Natural History	75
New York Prison Association	1
University of the City of New York	3
R. Van Arsdale	1
George Bancroft	2
Dr. J. W. Draper	6
Dr. Daniel Eaton	2
Dr. Geabel	1
Henry Grinnell	5
A. R. Grote	1
Mr. Harlan	1
Dr. Jacoby	1
Dr. Lieber	1
General T. C. de Mosquera	3

D.—*Addressed packages received by the Smithsonian Institution, &c.*—Continued.

	No. of packages.
NEW YORK, N. Y.—Continued.	
Charles B. Norton	3
Edward Norton	3
Baron Osten Sacken, (consul general, Russia)	3
Dr. Schwab	1
Dr. Limrock	1
Dr. G. Thurber	1
Dr. John Torrey	2
NORTHAMPTON, MASSACHUSETTS.	
State Lunatic Asylum	1
T. Lyman	8
OLYMPIA, WASHINGTON TERRITORY.	
Territorial Library	4
OMAHA, NEBRASKA.	
State Library	4
OWEGO, NEW YORK.	
R. Pumpelly	5
PEORIA, ILLINOIS.	
Dr. F. Brendel	1
PHILADELPHIA, PENNSYLVANIA.	
Academy of Natural Sciences	160
American Philosophical Society	98
Central High School	6
Central High School Observatory	3
Entomological Society of Philadelphia	3
Franklin Institute	11
Girard College	1
Historical Society of Pennsylvania	4
Pennsylvania Horticultural Society	1
Pennsylvania Institution for the Blind	2
Pennsylvania Hospital for the Insane	1
Pharmaceutical Association	6
Philadelphia Library Company	4
Wagner Free Institute	7
H. C. Carey	2
Pliny Earl Chase	1
E. D. Cope	3
Elias Durand	2
Dr. Otto Köhnig	1
Dr. Isaac Lea	11
Dr. John L. Leconte	10
Dr. Joseph Leidy	8
Dr. J. Aitken Meigs	2
F. Packard	1
W. B. Reed	1
E. Robinson	1
Dr. W. Sharswood	2

	No. of packages.
PHILADELPHIA, PA.—Continued.	
Dr. Slack	1
Prof. Strier	1
G. W. Tryon	4
Prof. Wagner	1
Samuel S. White	1
PRINCETON, NEW JERSEY.	
College of New Jersey	1
Prof. A. Guyot	2
PROVIDENCE, RHODE ISLAND.	
Brown University	2
Butler Hospital for Insane	1
Rhode Island Historical Society	1
State Library	5
Prof. Caswell	1
QUEBEC, CANADA.	
Literary and Historical Society	1
SACRAMENTO, CALIFORNIA.	
State Library	5
SALEM, MASSACHUSETTS.	
Essex Institute	2
SAN FRANCISCO, CALIFORNIA.	
California Academy of Natural Sciences	33
W. P. Blake	2
Dr. Hays	1
ST. LOUIS, MISSOURI.	
Deutsche Institut für Beförderung der Wissenschaiten	2
St. Louis Academy of Sciences	126
University	3
Dr. G. Engelmann	16
Dr. Albert Koch	2
Dr. B. F. Shumard	8
G. C. Swallow	1
ST. PAUL, MINNESOTA.	
Historical Society of St. Paul	1
SING SING, NEW YORK.	
Dr. George J. Fisher	2
SPRINGFIELD, ILLINOIS.	
State Agricultural Society	2
State Library	5

D.—*Addressed packages received by the Smithsonian Institution, &c.*—Continued.

	No. of packages.		No. of packages.
STOCKTON, CALIFORNIA.		WASHINGTON, D. C.—Continued.	
State Insane Asylum	2	War Department	3
		Prof. A. D. Bache	43
TAUNTON, MASSACHUSETTS.		Prof. S. F. Baird	7
		Miss Dorothea Dix	1
State Lunatic Hospital	1	Colonel W. H. Emory	2
		J. Ferguson	2
TOPEKA, KANSAS.		Theo. Gill	4
		Captain J. M. Gilliss, United States navy	28
State Library	4	Dr. F. V. Hayden	1
TORONTO, CANADA.		J. Hitz, (consul general, Switzerland)	2
		Prof. Hubbard	1
Canadian Institute	4	General Humphreys	1
Observatory	1	Prof. W. E. Jillson	2
		J. C. G. Kennedy	6
TRENTON, NEW JERSEY.		Admiral S. P. Lee	2
		F. B. Meek	1
State Lunatic Hospital	1	H. R. Schoolcraft	8
State Library	4	Dr. W. Stimpson	7
		John Willing	1
UTICA, NEW YORK.		Prof. Winlock	1
		John Xantus	1
State Lunatic Asylum	2		
WASHINGTON, D. C.		WILLIAMSBURG, VIRGINIA.	
Bureau of Ordnance and Hydrography	3	Eastern Lunatic Asylum	1
Department of Agriculture	2		
Engineer Bureau	1	WORCESTER, MASSACHUSETTS.	
Ordnance Bureau	4		
Library of Congress	11	American Antiquarian Society	8
Secretary of War	6	Catholic College	1
Surgeon General, United States army	7	State Lunatic Hospital	1
United States Coast Survey	30		
United States Naval Observatory	92	Total of addresses	299
United States Patent Office	149	Total of parcels	2,482

MUSEUM AND COLLECTIONS.

Although the additions to the collections have not been as numerous as in some preceding years, their value has been considerable, as consisting principally of new material from comparatively little known portions of America. A detailed list of donations and additions to the museum will be found at the end of the present report. The following are the principal regions and sources from which collections have been received.

Arctic America.—Very large collections of mammals, birds, eggs, &c., made in the northern parts of British America during 1863 and 1864, and filling 29 cases, reached Fort Garry in September last, and were forwarded to St. Paul, but arrived there after navigation had closed. They are now on their way to Washington by wagon and railroad, and are expected to arrive in a few weeks. They embrace many species not previously received from the north, and were collected principally by Messrs. MacFarlane, Lockhart, Jones, Sibbiston, Gaudet, Flett, Reid, Mactavish, Gunn, &c., already well known from repeated mention in previous reports. A full account of the collection will, it is hoped, be presented in the report for 1865.

From Labrador small but interesting and instructive collections of birds, with some eggs, were received from Mr. Henry Connolly and Mr. B. Smith; and from Moose Factory, from Mr. John MacKenzie, all of the Hudson's Bay service.

Pacific coast of the United States.—Mr. J. G. Swan, of Puget sound, has continued his important transmissions illustrating the zoölogy and ethnology of that region. Professor Whitney, of the geological survey of California, has also sent to the Institution many specimens collected by Dr. J. G. Cooper, zoölogist of the survey, to be identified by comparison with the Smithsonian types.

Interior region of the United States.—Assistant Surgeon Elliot Coues and Acting Assistant Surgeon J. A. Beers, having been ordered to report to the military commander of New Mexico for duty, left early in the spring for Santa Fé. During their journey they made valuable collections of birds, which were received in good condition. On reaching Santa Fé, Dr. Coues was ordered to Fort Whipple, near Prescott, the newly established capital of Arizona, which he reached in August, and where he has been diligently engaged, in the intervals of his official duties, in exploring the natural history of that interesting and little-known region. The results of his labors up to the beginning of November, filling three boxes, have reached San Francisco, and have been shipped to the Institution, where they may be expected shortly to arrive. Dr. Beers has been stationed at Fort Goodwin, on the Gila, and is also making collections there, none of which, however, have yet been received.

In April last, Acting Assistant Surgeon R. Hitz left Washington for Fort Laramie, to serve as surgeon to a projected western expedition from that post, under Colonel Collins. During his term of service, Dr. Hitz made large collections of specimens, principally on Laramie Peak, which have not yet arrived, owing to the freezing up, in the Missouri river, of the steamboat upon which they had been shipped.

In May last, Captain John Feilner, 1st United States cavalry, was detailed to accompany the expedition of General Sully, fitted out to control the Sioux Indians of the Upper Missouri, and during his stay at Sioux City, in the month of May, collected and forwarded a valuable collection of birds. He then proceeded to Fort Rice, a new post at the mouth of Cannon-ball river, and afterwards started with the command on the westward expedition. On the 30th of June, when about 100 miles from Fort Rice, Captain Feilner was ambushed by hostile Indians and mortally wounded, surviving only a few hours. In this untimely death of Captain Feilner the Institution loses one of its most valued correspondents. Many previous Reports bear testimony to the services he has rendered to science by his numerous collections of specimens in natural history, prepared with unusual skill, and made in the—till then—unknown regions about Fort Crook, California.

Eastern United States.—Extensive collections of eggs have been received from Dr. William Wood, of Connecticut, Mr. J. W. Tolman, of Illinois, and Dr Hoy, of Wisconsin. Dr. F. V. Hayden and Dr. Craven have furnished series illustrating the invertebrate fauna of the coast of South Carolina. Captain William Holden, of the quartermaster's department, supplied a large number of serpents and other reptiles from the vicinity of Newbern, North Carolina.

Mexico.—The most important collections received from Mexico, during the year, have been those of Mr. Xantus. Mention was made in a previous report of his operations about Colima and Manzanillo and vicinity, and of specimens received from him. The remainder of the collections of 1863 were received in 1864, and filled 28 boxes, (two others still deficient,) consisting of a general assortment of the land and aquatic animals, numerous minerals, and a few plants of the region investigated, and embracing much that is new to science. These collections were due early in the year, but were detained several months in Manzanillo by the blockade of the port by the French; and were

finally brought to Panama on the United States steamer Narragansett, and delivered to the Panama Railroad Company, by the kindness of Commander S. E. Woodworth, United States navy.

Several collections of birds and their eggs from the vicinity of Mazatlan have been presented by Colonel A. J. Grayson, an old correspondent of the Institution. These have proved of much interest, as showing an extension northward, along the coast, of the Central American fauna, of much greater extent than formerly supposed. Colonel Grayson is at present engaged in exploring the ornithology of the large islands, Three Marias, &c., off the coast of Mexico, and will doubtless make some interesting discoveries.

Additional collections of birds and other animals, as well as of plants, have been received from Dr. Charles Sartorius, of Mirador, a gentleman to whom the Institution already owes so much material relative to the natural history of Mexico. Collections of birds, mammals and reptiles, in continuation of previous ones, have also been presented by Dr. Sumichrast, of Orizaba.

Dr. H. Berendt, long resident in Tabasco, visited the United States early in the year, bringing with him valuable collections of natural history for the Institution. These consisted principally of reptiles, among them a complete series of the Chelonia, found near Tabasco.

At the present time, an extensive exploration of the physical and natural history of Yucatan is in progress, of which the Institution expects some of the results. It is under the direction of Señor Salazar y Llarregui, well known as former Mexican commissioner of the United States and Mexican boundary survey. Dr. Arthur Schott, of whom frequent mention has been made in previous reports, as naturalist of the United States and Mexican boundary survey, Lieutenant Michler's survey of the Atrato river, &c., has been attached, in a similar capacity, to the Yucatan survey.

Central America.—An extensive and most important series of birds of Guatemala has been received from Mr. Osbert Salvin, of London, types of the report made on the Guatemalan collections by himself and Mr. F. Godman. A valuable collection of Costa Rican birds has been received from Dr. A. von Frantzius, already referred to in previous reports; and many specimens, also, from Mr. J. Carmiol; the two series embracing an unusual proportion of new and rare species.

From Captain J. M. Dow additional collections of birds of the coast region of Central America have been received; and several collections made by Mr. Frederick Hicks have been submitted to my examination.

South America.—Specimens of birds from Bogota have been presented by Mr. J. Krider, Mr. J. Akhurst, and Mr. J. H. Roome. Additional collections of birds of Ecuador, of much value, have been received from Hon. J. R. Buckalew and Prof. W. Jameson; and, also, quite a number from Mr. Akhurst. A collection of birds from the vicinity of La Paz, Bolivia, made and presented by Hon. D. K. Cartter, has proved of the greatest interest, embracing, among other novelties, two new humming birds. A collection of woods from Surinam has been received from Mr. Henry Sawyer.

West Indies.—Additional collections from Mr. W. T. March, of Jamaica, and from Mr. Charles Wright, Mr. N. H. Bishop, and Dr. F. Gundlach, of Cuba, have tended to make the Smithsonian series of birds from these islands still more complete than heretofore. A valuable collection of shells of Jamaica has also been received from Mr. March, and of Cuba from Mr. Wright.

A series of birds of Nassau, New Providence, presented by Lieutenant Fitzgerald, of the British army, included several species new to the collection.

In addition to the collections of birds referred to above, as received from particular regions, Mr. Ed. Verreaux, of Paris, has presented, through the instrumentality of Mr. Jules P. Verreaux, the eminent ornithologist, a large number of species from Mexico, Central and South America, embracing many

valuable types, and of inestimable service in prosecuting the study of Middle and South American birds, only equalled in this respect by the donations of Mr. Salvin.

As might be expected from the plan of operations of the Institution, the collections received from Europe and the rest of the Old World, during the year, are much inferior in number and extent to those from America. Among these, however, should be especially mentioned a series of Scandinavian Ptarmigans and of rare northern birds, from Prof. C. Sundevall, of Stockholm; and of rare European eggs, from the Royal Artillery Institution, of Woolwich; Prof. Haidinger and Prof. Hörnes, on the part of the K. K. Geologische Reichsanstalt, in conjunction with the Imperial Mineralogical Museum of Vienna, have presented an extensive series of fossils of the tertiary basin of Vienna. The K. Ober-Bergamt, of Breslau, has contributed a very useful series of minerals and mining products, illustrating the metallurgy of the royal mining establishment of Silesia.

INVESTIGATION, IDENTIFICATION, AND ENTERING OF COLLECTIONS.

Steady progress has been made, during the year, in the determination and labelling of the collections of the Institution, and the setting aside of the duplicates, by the different gentlemen to whom specimens have been intrusted for the purpose. I have, myself, been engaged, at moments not otherwise occupied, in reviewing the entire collection of birds of America, both north and south, in charge of the Institution, and in preparing a descriptive catalogue for publication in the Smithsonian Miscellaneous Collections. Of this review about one hundred and fifty pages have been printed, and an additional portion is in press. I am indebted to several gentlemen for important aid in carrying out this undertaking, by the loan of specimens necessary for comparison; especially to Mr. George N. Lawrence, of New York; Mr. John Cassin, of Philadelphia; Dr. Samuel Cabot, jr., of Boston; and Messrs. Edward and Alfred Newton and Mr. O. Salvin, of England.

Mr. W. S. Morgan, of Washington, has been systematically engaged in making a series of drawings of all the varieties of authenticated species of North American eggs in the collection, to be used in preparing the illustrations to Dr. Brewer's work on North American Oölogy.

The following table shows the amount of work done, during the year, in the way of registering the collections.

Table showing the total number of entries on the record-books of the Smithsonian Collections at the end of the years 1863 *and* 1864.

	1863.	1864.
Skeletons and skulls	5,614	6,275
Mammals	7,175	7,782
Birds	31,800	35,111
Reptiles	6,325	6,543
Fishes	5,075	5,404
Eggs of birds	7,275	8,700
Crustaceans	1,287	1,287
Mollusks	10,450	10,525
Radiates	2,714	2,725
Fossils	2,550	5,487
Minerals	4,476	4,925
Ethnological specimens	875	1,048
Annelids	110	110
Total	85,726	95,922

If to the above we add the mollusca catalogued, by Mr. Carpenter, the record of which, filling an entire volume, is still in his hands, it will bring the total number of entries fully up to 100,000. As in all departments, excepting those of mammals, birds, and osteology, each entry may include a large number of specimens, it is a fair allowance to estimate an average throughout of five to each number, making half a million of objects catalogued (probably many more) and marked by permanent labels or figures. The average number of entries for the twelve years during which the system has been continued will thus exceed 8,000.

DISTRIBUTION OF DUPLICATES.

A very large number of specimens have been issued from the duplicate stores of the Institution, in 1864, both to public museums at home and abroad, and to gentlemen requiring them for special investigations. The following table exhibits the statistics of distribution as far as they could be reduced to figures:

	Species.	Specimens.
Mammals	168	665
Birds	1,490	2,708
Eggs	431	1,641
Reptiles	51	69
Shells	*600	*2,100
Crustaceans	77	622
Marine invertebrates generally	1,072	3,798
Casts of antiquities	58	58
Rocks	*500	*1,000
Building stones	*360	3,600
Total	4,807	16,461

The estimate, although somewhat approximate, is under the true amount of material distributed, rather than in excess. Every specimen included in it was carefully and authentically labelled before being issued. Of the birds referred to, 360 were mounted duplicates of the United States Exploring Expedition, supplied to the Boston Society of Natural History and the Philadelphia Academy of Natural Sciences.

PRESENT CONDITION OF THE MUSEUM.

The specimens exhibited in the museum are all, apparently, in good condition, free from insects, and nearly all properly identified and labelled. The series of fishes has been removed from the rooms under the art-gallery and placed in the northwest gallery of the museum hall, arranged for their reception. A series of casts of interesting Mexican masks and other antiquities, from originals in the museum of the American Philosophical Society, has been placed in the eastern end of the northeast gallery.

A large portion of the collections of the exploring expedition and Western American shells, intrusted to Mr. Carpenter for determination, has been returned by him, but they remain in the cases in which they arrived until the rest shall have been received, by request of Mr. Carpenter.

*Approximate number.

LIST OF DONATIONS TO THE MUSEUM DURING 1864.

——. ——.—Box of fossils, Mount Pleasant, Iowa.
Abert, Thayer.—Four cases of minerals, (deposited.)
Adams, W. H.—Indian arrow-head from Illinois.
Akhurst, J.—Collection of birds of New Granada and Cuba. Skins of birds of Ecuador.
Aldrich, T.—Shells and insects from New York.
Andrews, Prof. E. B.—Meteorite from Ohio; alcoholic specimens, &c.
Austin, E. P.—Insects from Michigan.
Babcock, A. L.—Skins of birds from Massachusetts.
Beebe, E. H.—Minerals from Galena, Illinois.
Beers, Dr. H. A.—Collection of birds from Kansas and New Mexico.
Berendt, Dr. H.—Specimens of *Physella Berendtii* from Mexico. Reptiles, insects, medicinal plants, &c., from Tabasco, Mexico.
Berthoud, Lieutenant E. T.—Tooth of mastodon from Kansas.
Bertineau, Charles.—Ammonites and teeth of sharks and saurians from Pembina river.
Bishop, N. H.—Box of birds from Remedios.
Blake, J. S.—Four boxes of gold and silver ores from Agua Blanca, Mexico.
Bland, Thomas.—Box of West Indian shells.
Boardman, G. A.—Skins of birds; nest of raven from New Brunswick.
Brawner, I. C.—Specimen of monstrosity.
Breslau, K. Ober Berg-Amt.—Five boxes of minerals and metallurgical specimens from the royal mining establishment in Silesia.
Buchner, Dr. O.—Six boxes of models of German fungi.
Buckalew, Hon. C. R.—Skins of birds from Ecuador.
Campfield, H.—Surgical knife used in the revolutionary war.
Carmiol, Julian.—Two boxes of birds and mammals from Central Costa Rica.
Carpenter, W. T.—Collection of Australian polyzoa.
Cartter, Hon. D. K.—Series of birds from La Paz, Bolivia.
Christiania, Ethnological Museum of.—Illustrations of Lapland ethnology.
Collins, Colonel W. O.—Skins of *Leucosticte tephrocotis* and *Lepus Townsendii*, Fort Laramie.
Connolly, Henry.—Box of birds from Labrador.
Cooper, Dr. J. G.—Skull of *Vulpes littoralis*, San Nicolsar island, California.
Cope, Prof. E. D.—Box of fossil ganoid fishes from England.
Cosgrove, Lawrence.—Iron pyrites from Fort Scott, District of Columbia.
Coues, Dr. Elliot.—Two boxes of birds from Kansas and New Mexico.
Craven, Dr.—Box of mollusca from Hilton Head.
Dall, W.—Insects and shells of Massachusetts.
Davis, H.—Box of fresh-water shells of Iowa.
Day, H. H.—Silver ores from the Savage mine, Virginia city.
Dobson, W.—Arrow-heads, Crown Point, New York.
Dold, Andres.—Tooth of elephant, Las Vegas, New Mexico.
Dow, Captain J. M.—Box of birds from Central America. Skin of Jaguar, Central America.
Fitzgerald, Lieutenant C. L.—Birds and shells of the Bahama islands.
Flint, Dr. Earl.—Orthopterous insect from Nicaragua.
Foot, Hon. S.—*Octopus* from Fort Pickens; collected by Captain H. A. Smalley. Ores of iron from Vermont.
Foster, Colonel J. W.—Cloth from an ancient mound in Ohio.
Frantzius, Dr. A. von.—Series of birds of Central Costa Rica.
Galody, Hon. M.—Insects and crabs from Dominica.

Getter, O. H.—Specimen of *Reduvius.*
Gill, Theodore.—Skins of humming birds, reptiles, shells, &c., West Indies.
Gilpin, Dr. J. B.—Shrews and mice of Nova Scotia, in alcohol.
Grayson, Colonel A. J.—Collection of birds and eggs from vicinity of Mazatlan.
Gundlach, Dr. J.—Mounted birds from Cuba.
Haidinger, Prof. W.—See Vienna.
Hall, John H.—Specimens of Brucite from Lancaster county, Pennsylvania.
Hayden, Dr. F. V.—Three boxes of alcoholic collections from Beaufort, South Carolina.
Hepburn, James.—Box of birds from British America.
Heramb, Carl.—Shot pouch of eel skin from Norway.
Holden, Captain W.—Three kegs of serpents from Newbern, North Carolina.
Hörnes, Dr.—See Vienna.
Hoy, Dr. P. R.—Box, nest and eggs, Wisconsin.
Jameson, Prof. W.—Skins of *Tétragonops rhamphastinus* and *Merulaxis orthonyx* and other birds from Ecuador.
Jouett, U. S. N., Captain J. E.—Skin of alligator gar.
Krefft, Dr. G.—Scale of Osteoglossoid fish, collected in Australia by Dr. Leichhardt.
Krider, John.—Skins of South American birds. Six skins of birds from Bogota.
Lincoln, C. D.—Birds' eggs from Taunton.
Lippincott, B.—Mosses and beaver-tail from Oregon.
Mackenzie, John.—Box of birds from Moose Factory.
March, W. T.—Shells, with skins and eggs of birds from Jamaica.
Maximilian, Prince of Wied.—Box of European birds.
Meigs, Major General M. C.—Timber bored by *Teredo.*
Merrit, B. A.—Shells, lichens, and skin of *Arvicola pinctorum* from New York.
Odell, Franklin.—Indian relics and quills of porcupines from New Hampshire.
Paine, C. S.—Skin of albino mouse and of *Arvicola albo-rufescens (?)* from Vermont. Skins of Bonaparte's gull, and nest of *Turdus pallasii.*
Palmer, Dr. E.—Box of plants, alcoholic specimens, &c., from Kansas. Three boxes of minerals, plants, zoölogical specimens, Colorado Territory.
Parkinson, D. F.—Six Indian skulls from California.
Pease, W. H.—Two boxes of Sandwich island shells.
Philadelphia Academy of Natural Sciences.—Duplicates of South American birds.
Poole, Henry.—Shells and birds' eggs, Cape Breton.
Prentiss, Dr. D. W.—Birds of Hilton Head, South Carolina. Nest of marsh wren.
Roome, J. H.—Collection of Bogotan birds.
Rowell, Rev. J.—Box of California shells.
Saemann, L.—Series of specimens of rocks.
Samuels, E. A.—Eggs of summer duck and of hooded merganser. Nests of birds from Maine.
Sartorius, Dr.—Birds, plants, and shells from Mirador, Mexico.
Sawyer, Henry.—One hundred species of wood from Dutch Guiana.
Shimer, Henry.—Birds from Illinois.
Shute, James G.—Nest and eggs of *Dendroica pinus.*
Siler, Andrew L.—Fossil wood, shells, and Indian curiosities, Utah.
Simpson, George B.—Copper spear-head and other relics.
Smalley, Captain H. A.—*Octopus* from Fort Pickens, Florida.

Smith, B.—Box of birds from Labrador.

Sprague, E. T.—Minerals from Essex county, New York.

Stimpson, Dr. W.—Zoölogical collections from Beesley's Point.

Strong, C.—Silver ores from the Gould & Curry mine, Virginia city.

Sumichrast, Prof. F.—Collection of vertebrata from Mexico.

Sundevall, Prof.—Skins of birds of Sweden.

Scott, Ansel.—Indian relics.

Swan, Jas. G.—Three boxes zoölogical collections and Indian curiosities from Puget Sound.

Taylor, A. S.—Grasshoppers from California.

Thomson, J. H.—*Motella caudacuta* from New Bedford. Mollusca from the coast of Massachusetts.

Tolman, J. W.—Collection of eggs from Illinois.

Velie, Dr. J. W.—Egg of *Mergus cucullatus* and *Ectopistes migratoria.*

Verreaux, Edward.—Three hundred specimens of birds and ten skins of mammals from Central and South America.

Vienna Geologisches Reichsanstalt, and the Imperial Mineralogical Museum.—Six hundred species of tertiary fossils of the Vienna basin, furnished by Prof. Haidinger and Prof. Hörnes.

Vienna Kais. Mineralogisches Museum.—See Geologisches Reichsanstalt.

Walker, R. L.—*Myriapoda* from Alleghany County, Pennsylvania.

Warren, Major General G. K.—Skin of *Actodromas maculata*, Petersburg.

Winslow, Dr. C. F.—Skin of *Daption capensis*, Peru.

Wharton, Thomas.—Ores of Nickel, nickel and copper coins, &c.

Whitney, Prof. J. D.—Zoölogical collections of Geological Survey of California. (Deposited.)

Wood, Dr. W.—Box of eggs from Connecticut.

Woolwich, Royal Artillery Institution.—Eggs of European birds.

Wright, Charles.—Birds and shells of Cuba. Box of Cuban land shells.

Xantus, J.—Numerous boxes zoölogical collections, Manzanillo, Colima, Mexico.

Young, Prof. C. A.—Specimen of Parmellee Meteorite of February 28, 1858, and of the Meteorite of New Concord, Guernsey county, Ohio, May 1, 1860.

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Cambridge Observatory	1
University Library	1
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Natural History Society of Dublin	1
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Royal Irish Academy	12
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Meteorological Society of Scotland	16
Royal College of Physicians	1

Royal Observatory	1
Royal Physical Society	3
Royal Scottish Society of Arts	2
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Glasgow.—University Library	6
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Kilkenny.—Kilkenny and Southeast of Ireland Archæological Society	23
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Leeds Philosophical and Literary Society	28
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Schmid, Dr. E. E.	3	Zuchold, A.	5

LIST OF METEOROLOGICAL STATIONS AND OBSERVERS

OF THE

SMITHSONIAN INSTITUTION

FOR THE YEAR 1864.

Name of observer.	Station.	North latitude.	West longitude.	Height.	Instruments.*	No. of months received.
BRITISH AMERICA.		° ′	° ′	*Feet.*		
Acadia College	Wolfville, Nova Scotia	45 06	64 25	95	A	8
Baker, J. C	Stanbridge, Canada East	45 08	73 00		T	12
Clarke, Lawrence, jr	Fort a la Corne, Saskatchewan				T	2
Delaney, Edward M. J	Colonial Building, St. John's, Newfoundland.	47 35	52 40	170	B. T. R	2
Magnetic Observatory	Toronto, Canada West	43 39	79 21	†108	A	12
Murdock, G	St. John, New Brunswick				B. T. R	7
Rankin, Colin	Michipicoton, Canada West	47 56	85 06	660	B. T	12
MEXICO.						
Laszlo, Charles	San Juan Bautista, Tabasco	17 47	92 36	40	A	7
Sartorius, Dr. Charles	Mirador, Vera Cruz	19 15	96 25	3,600	A	10
Nieto, José A	Cordova, Vera Cruz	18 54			B. T. R	12
CENTRAL AMERICA.						
Riotte, C. N	San José, Costa Rica	9 54	84 06	3,772	T. R	11
White, William T., M. D	Aspinwall	9 21	79 54		A	6
WEST INDIES.						
United States Consul	Turk's Island	3 00				7
Julien, Alexis A	Sombrero Island	18 35	63 27	45	A	3
BERMUDA.						
Royal Engineers, (in the Royal Gazette.)	Centre Signal Station, Saint George's				A	12
SOUTH AMERICA.						
Hering, C. T	Government Plantation Rustenberg, colony of Surinam, Dutch Guiana.				A	6

* A signifies Barometer, Thermometer, Psychrometer, and Rain Gauge.
B signifies Barometer.
T signifies Thermometer.
P signifies Psychrometer.
R signifies Rain Gauge.
N signifies no instrument.
† Above Lake Ontario.

List of meteorological stations and observers, &c.—Continued.

Name of observer.	Station.	County.	North latitude.	West longitude.	Height.	Instruments.	No. of months received.
CALIFORNIA.			° ′	° ′	*Feet.*		
Canfield, Colb't A., M. D.	Monterey	Monterey	36 36	121 54	40	T. P. R.	10
	Meadow Valley	Plumas	40 15	120 15	3,700	T. P. R.	2
Hays, W. W., M. D.	Santa Barbara	Santa Barbara	34 35	119 40	30	B. T. R.	8
Logan, Thomas M., M. D.	Sacramento	Sacramento	38 35	121 28	41	A.	7
Parkinson, David F.	Presidio of San Francisco.	San Francisco	37 48	122 22	....	A.	9
Smith, Mrs. M. D.	Spanish Ranche.	Plumas	39 56	120 40	3,700	B. T. R.	12
COLORADO.							
Luttrell, James	Montgomery	Park	39 00	106 00	13,000	T.	5
CONNECTICUT.							
Hunt, Rev. Daniel	Pomfret	Windham	41 52	72 23	567	A.	12
Johnston, Prof. John	Middletown	Middlesex	41 32	72 39	175	A.	12
Learned, Dwight W.	Plymouth	Litchfield	41 40	73 03	....	T.	5
Leavenworth, D. C.	New Haven	New Haven	41 18	72 56	40	B. T.	4
Rockwell, Charlotte	Colebrook	Litchfield	42 00	73 06	....	T.	7
Yeomans, William H.	Columbia	Tolland	41 40	72 42	....	T.	12
DELAWARE.							
Hedges, Urban D., M. D.	Wilmington	New Castle	39 47	75 33	115	T. R.	12
DISTRICT OF COLUMBIA.							
Smithsonian Institution	Washington	Washington	38 53	77 01	60	A.	12
FLORIDA.							
Dennis, William C.	Key West	Monroe	24 33	81 28	16	B. T. R.	2
IDAHO.							
Collins, Col. W. O.	Fort Laramie	....	42 10	104 47	4,472	T.	9
Finfrock, J. H., M. D.	Fort Halleck	....	....	....	....	T.	4
ILLINOIS.							
Adams, W. H.	Elmore	Peoria	....	....	....	R.	6
Aldrich, Verry	Tiskilwa	Bureau	41 15	89 66	550	T.	12
Babcock, E.	Riley	McHenry	42 11	88 20	760	T. R.	12
Ballou, N. E., M. D.	Sandwich	De Kalb	41 31	88 30	665	T. R.	12
Bandelier, Adolphus F., jr.	Highland	Madison	38 45	89 46	....	B. T. P.	3
Brendel, Frederick, M. D.	Peoria	Peoria	40 43	89 30	460	A.	12
Brookes, Samuel	Chicago	Cook	42 00	87 30	600	T.	12
Byrne, Arthur M.	Chicago	Cook	41 57	87 38	591	B. T.	1
Dudley, Timothy	Jacksonville	Morgan	39 30	90 06	676	T. R.	12
Ellsworth, J.	Hoylton	Washington	38 30	89 00	....	T. R.	10
Grant, John; Grant, Miss Ellen	Manchester	Scott	39 33	90 34	683	A.	12
Griffing, Henry.	Hazel Dell	Cumberland	39 00	88 00	....	N.	12
Langworthy, A. D.	Evanston	Cook	42 03	87 38	588	B. T.	1
Livingston, Prof. William	Galesburg	Knox	....	....	....	A.	12
Mead, S. B., M. D.	Augusta	Hancock	40 10	91 00	*203	T. P. R.	12
Merwin, Mrs. Emily H.	Ottawa	La Salle	41 20	88 47	500	T. R.	11
Morrison, William H.	Evanston	Cook	42 02	87 38	590	B. T.	3
Moore, C. H.	Clinton	De Witt	40 09	88 58	....	B. T.	1
Phelps, E. S.	Wyanet	Bureau	41 30	89 45	....	T. R.	4
Riblet, J. H.	Pekin	Tazewell	40 36	89 45	....	B. T. R.	12
Scovill, Homer W.	Evanston	Cook	42 02	87 38	570	B. T. R.	2
Tolman, James W.	Winnebago Depot.	Winnebago	42 17	89 12	900	B. T. R.	12
Trible, Mrs. Anna C.	Upper Alton	Madison	39 00	89 36	....	A.	2
INDIANA.							
Anderson, Henry H.	Rockville	Parke	36 00	87 00	1,100	T. R.	1
Burroughs, Reuben	South Bend	St. Joseph	41 39	86 71	600	T. R.	12
Boerner, Charles G.	Vevay	Switzerland	38 46	84 59	....	T. R.	3
Butterfield, W. W.	Indianapolis	Marion	39 45	86 20	698	T.	3

* Above low-water mark at Quincy.

List of meteorological stations and observers, &c.—Continued.

Name of observer.	Station.	County.	North latitude.	West longitude.	Height.	Instruments.	No. of months received.
INDIANA—Continued.			° ′	° ′	*Feet.*		
Chappellsmith, John	New Harmony	Posey	38 08	87 50	320	A.	11
Collins, Rev. Samuel	Madison	Jefferson	38 45	83 40		B. T. R.	3
Crozier, Dr. E. S	New Albany	Floyd	38 02	85 29	353	B. T. P.	12
Dawson, William	Spiceland	Henry	39 48	85 18	1,025	B. T. R.	12
Griest, John	Pennville	Jay	41 30	85 00		T.	8
Hobbs, William Henry	Bloomingdale	Parke	39 48	87 00	*150	N.	6
Loughridge, J. H., M. D.	Rensselaer	Jasper				T. R.	6
Mayhew, Royal	Indianapolis	Marion	39 55	86 00	698	T. R.	12
Redding, Thomas B.	Newcastle	Henry	39 55	85 27	1,000	B. T.	12
Rice, E. J	Muncie	Delaware	40 12	85 20		B. T. R.	7
IOWA.							
Briggs, Rev. E. L	Mount Pleasant	Henry	41 00	91 38		T. R.	8
Chamberlain, John	Davenport	Scott	41 30	90 40	737	A.	12
Collin, Prof. Alonzo	Mount Vernon	Linn	42 00	91 00		T.	12
Deering, D. S	Independence	Buchanan	42 30	92 16	850	T.	11
Dorweiler, Philip	Guttenburg	Clayton				T. R.	5
Doyle, L. H	Waterloo	Black Hawk	42 30	92 31		T. R.	1
Farnsworth, P. J., M. D.	Lyons	Clinton	41 50	90 10	401	T. R.	11
Foster, Suel	Muscatine	Muscatine	41 26	92 00		N.	1
Horr, Asa, M. D	Dubuque	Dubuque	42 30	90 52	666	A.	12
McConnel, Townsend	Pleasant Plain	Jefferson	41 07	94 54	950	T. R.	11
McCoy, Franklin, M. D. McCoy, Miss Elizabeth	Algona	Kossuth	43 01	94 04	1,500	T. R.	12
McCready, Daniel	Fort Madison	Lee	40 37	91 28		T. R.	12
Mead, Chauncey	Monticello	Jones	42 13	91 15	880	T. R.	6
Parvin, Prof. Theodore S.	Iowa City	Johnson	41 37		621	A.	12
Stebbins, Richard	Onowa	Monona	42 00	96 11		B. T.	8
Steed, F	Waterloo	Black Hawk	42 30	92 31		T. R.	7
Townsend, Nathan	Iowa Falls	Hardin	42 32	93 20		T. R.	12
Walton, Josiah P.	Muscatine	Muscatine	41 25	92 02	582	A.	10
Wheaton, Alex. Camp	Independence	Buchanan	42 29	91 50		T. R.	12
KANSAS.							
Beckwith, W.	Olatha	Johnson	38 50	94 43		T.	11
Drew, F. P., M. D., U.S.A.	Fort Riley		39 00	96 30	1,300	T. R.	12
Fuller, Arthur N.	Lawrence	Douglas	38 58	95 13	970	R.	2
Denison, Henry L.	Manhattan	Riley	39 13	96 45	1,000	T. R.	11
Soule, W. L. G	Lawrence	Douglas	38 58	95 13	970	R.	1
KENTUCKY.							
Caldwell, R. H	Danville	Boyle				B.	2
Young, Mrs. Lawrence	Louisville	Jefferson	38 07	85 24	570	A.	12
MAINE.							
Brackett, Geo. Emerson	Belfast	Waldo	44 23	69 08		T. R.	1
Dana, Wm. D	North Perry	Washington	45 00	67 06	100	A.	12
Gardiner, Rev. Frederick	Gardiner	Kennebec	44 41	69 46	90	B. T. R.	12
Guptill, G. W	Cornishville	York	43 40	70 44	800	T. R.	12
Moore, Asa P.	Lisbon	Androscoggin	44 00	70 04	130	T. R.	12
Osgood, Henry H.	Bluehill	Hancock	44 25	68 34	50	T. R.	1
Parker, J. D.	Steuben	Washington	44 44	67 50	50	A.	12
Pitman, Edwin	Sebec	Piscataquis				T.	4
	Lee	Penobscot				T.	7
Pitman, Mark	Foxcroft	Piscataquis	45 12	69 13		B. T.	1
West, Silas	Cornish	York	43 40	70 44	784	B. T. R.	12
Wilbur, Benjamin F.	West Waterville	Kennebec				T. R.	12
MARYLAND.							
Baer, Miss Harriott M.	Sykesville	Carroll	39 23	76 57	700	T. P. R.	12
Dutton, Prof. J. Russell	Chestertown	Kent	39 12	75 59		A.	7
Gillingham, Warrington	Union Bridge	Carroll				B. T. R.	1
Goodman, William R.	Annapolis	Anne Arundel	38 59	76 29	20	A.	12
Lowndes, Benjamin O.	Bladensburg	Prince George's	38 57	76 58	112	T. R.	8
Stephenson, Rev. James	St. Inigoes	St. Mary's	38 10	76 41	45	A.	1[illegible]
Tabb, Philip	Ellicott's Mills	Howard				T.	3

* Height above Wabash river.

List of meteorological stations and observers, &c.—Continued.

Name of observer.	Station.	County.	North latitude.	West longitude.	Height.	Instruments.	No. of months received.
MASSACHUSETTS.			° ′	° ′	Feet.		
Astronomical Observatory	Williamstown	Berkshire	42 43	73 13	686	B. T. R.	12
Barrows, N., M. D.	Sandwich	Barnstable	41 45	70 30	......	T. R.	12
Caldwell, John H.	Topsfield	Essex	......	......	......	T. R.	5
	Newbury	Essex	......	......	25	T.	7
Davis, Rev. Emerson	Westfield	Hampden	42 06	72 48	180	A.	12
Dewhurst, Rev. Eli	Baldwinsville	Worcester	42 37	72 05	847	B. T. R.	11
Fallon, John	Lawrence	Essex	42 42	71 11	133	A.	12
Merriam, Arthur M.	Topsfield	Essex	......	......	......	T. R.	7
Metcalf, John Geo., M. D.	Mendon	Worcester	42 06	71 34	......	B. T. R.	12
Prentiss, Henry C., M. D.	Worcester	Worcester	42 16	71 48	528	A.	6
Rodman, Samuel	New Bedford	Bristol	41 39	70 56	90	A.	12
Snell, Prof. E. S.	Amherst	Hampshire	42 22	72 34	267	A.	12
Tappan, Eugene	West Dennis	Barnstable	41 40	70 11	25	T. R.	2
MICHIGAN.							
Ellis, Edwin, M. D.	Garlick	Ontonagon	46 49	90 00	1,410	T. R.	6
Kedzie, Prof. R. C.	Lansing	Ingham	42 42	84 34	895	A.	12
Mapes, Henry H.	Oshtemo	Kalamazoo	......	......	......	N.	10*
Strong, L. H.	Holland	Ottawa	43 00	86 00	680	T. R.	7
Van Orden, William, jr.	Clifton	Keweenaw	47 00	88 00	800	T.	2
Weeks, James A.	Pontiac	Oakland	......	......	......	T.	10
Whelpley, Miss Florence E.	Monroe	Monroe	41 56	83 23	590	T. R.	12
Woodard, C. S.	Ypsilanti	Washtenaw	42 15	83 47	751	A.	9
MINNESOTA.							
Cheney, William	Minneapolis	Hennepin	45 00	93 10	856	B. T.	2
Graves, Mary A.	Tamarack	Hennepin	......	......	......	T.	7
Kilgore, William	Mankato	Blue Earth	44 08	93 30	50	T.	1
Paterson, Rev. A. B., D.D.	St. Paul	Ramsey	44 57	93 05	800	T. R.	12
Roos, Charles	New Ulm	Brown	44 16	94 26	850	T. R.	10
Smith, Henry L.	Forest City	Meeker	45 13	94 28	......	T. R.	1
Wieland, C.	Beaver Bay	Lake	47 17	91 18	650	B. T.	12
MISSISSIPPI.							
McCary, Robert	Natchez	Adams	31 34	91 25	264	B. T. R.	3
MISSOURI.							
Caldwell, J. T.	Athens	Clark	......	......	......	T.	8
Christian, John	Harrisonville	Cass	......	......	......	T.	12
Engelmann, George, M. D.	St. Louis	St. Louis	38 37	90 15	481	A.	8
Fendler, Augustus	St. Louis	St. Louis	38 37	90 16	470	B. T. P.	10
Zimmerman, John H., S. J.	St. Louis	St. Louis	38 40	90 15	475	A.	1
Muir, William	Laborville	St. Louis	38 33	90 43	......	T.	6
Ray, George P.	Canton	Lewis	40 12	91 37	......	T.	12
Ashley, P. B.	Easton	Buchanan	39 46	94 22	......	T. R.	4
NEBRASKA.							
Bowen, Miss Anna M. J.	Elkhorn City	Douglas	41 22	96 12	1,000	T.	12
Hamilton, Rev. William	Bellevue	Sarpy	41 08	95 50	......	T. R.	12
NEW HAMPSHIRE.							
Brown, Branch	Stratford	Coos	44 08	71 34	1,000	T. R.	12
Chase, Arthur	Claremont	Sullivan	43 22	72 21	539	B. T. R.	12
Mead, Stephen O.	Claremont	Sullivan	......	......	......	T.	7
Nason, Rev. Elias	Exeter	Rockingham	42 58	70 55	125	B. T.	12
Odell, Fletcher	Shelburne	Coos	44 23	71 06	700	B. T.	12
Pitman, Charles H.	North Barnstead	Belknap	43 38	71 27	......	T.	12
Sandt, Rufus	North Littleton	Grafton	44 20	71 50	......	B. T.	7
Whiting, Robert C.	Littleton	Grafton	44 20	72 00	......	T. R.	4
NEW JERSEY.							
Beans, Thomas J.	Progress	Burlington	40 03	75 11	15	T. R.	11
Brooks, William	Passaic Valley	Passaic	40 53	74 12	140	T. R.	6
Dunn, John C.	Burlington	Burlington	40 05	75 10	......	T. R.	8
Ingersoll, James S.	Cole's Landing	Camden	39 54	75 02	......	T.	12
Shaw, Morgan J., M. D.	Mount Holly	Burlington	39 59	74 47	30	B. T.	12

List of meteorological stations and observers, &c.—Continued.

Name of observer.	Station.	County.	North latitude.	West longitude.	Height.	Instruments.	No. of months received.
NEW JERSEY—Continued.			° ′	° ′	*Feet.*		
Sheppard, Clarkson	Greenwich	Cumberland	39 20	75 25	30	B. T. R.	12
Thompson, George W	New Brunswick	Middlesex	40 30	75 31	90	T.	8
Whitehead, W. A	Newark	Essex	40 45	74 10	35	B. T. R.	12
NEW YORK.							
Arden, Thomas B	Garrison's	Putnam	41 22	74 02	180	T. R.	12
Aubier, Rev. Jno. M., S. J	New York	New York	40 44	73 59		A.	9
Barrows, Storrs	South Trenton	Oneida	43 10	74 56	835	T. R.	10
Bartlett, Erastus B	Vermillion	Oswego	43 26	77 26	327	T.	12
Beauchamp, Wm. M	Skaneateles	Onondaga	43 00	76 30	932	B. T.	12
Bowman, John	Baldwinsville	Onondaga	43 04	76 41		T.	12
Cowing, Philo	Seneca Falls	Seneca	42 54	76 51	463	B. T.	7
Dill, John B	Auburn	Cayuga	42 55	76 28	650	T.	12
Denning, William H	Fishkill Landing	Dutchess	41 29	73 59	42	B. T. R.	12
Dewey, Prof. Chester	Rochester	Monroe	43 08	77 51	516	B. T. R.	12
Fuller, Robert M. Swart, Harmon V.	Schenectady	Schenectady	72 49	73 55		T.	12
Gardiner, James H	Newburg	Orange	41 31	74 01		B. T.	2
Gregory, S. O	Theresa	Jefferson	44 12	75 48	365	T. R.	12
Heimstreet, John W	Troy	Rensselaer	42 44	73 37	58	A.	2
Holmes, Dr. E. S	Wilson	Niagara	43 20	78 56	250	T.	12
Howell, Robert	Nichols	Tioga	42 00	76 32		T.	12
Hunt, Geo. M	North Argyle	Washington	43 00	72 29	290	B. T. R.	6
Hyde, Stephen	Palmyra	Wayne	43 04	77 20	466	T.	8
Ingalsbe, Grenville M	South Hartford	Washington	43 15	73 21	400	T. R.	12
Mack, Rev. Eli T	Flatbush	Kings	40 37	74 02	54	B. T. R.	12
McMore, P. A	Fort Ann	Washington	42 39	73 44	1,430	T. R.	10
Malcom, Wm. Schuyler	Oswego	Oswego	43 28	76 30	250	B. T. R.	12
Mathews, M. M., M. D	Rochester	Monroe	43 08	77 51	525	A.	12
Morris, Prof. Oran W	New York	New York	40 43	74 05	25	A.	12
Paine, H. M., M. D	Clinton	Oneida	43 03	75 15	600	T. P. R.	12
Pratt, Daniel J	Fredonia	Chautauqua	42 26	79 24		B. T. R.	2
Roe, Rev. San. W., M. D	Jamestown	Chautauqua	42 06	79 19	1,454	T. R.	11
Rogers, Francis M	Throg's Neck	Westchester	40 49	73 50	19	T. R.	12
Russell, Cyrus H	Gouverneur	St. Lawrence	44 19	75 29		B. T. R.	12
Smith, E. A	Moriches	Suffolk	40 49	72 36	13	T. R.	10
Spooner, Dr. Stillman	Oneida	Madison	43 04	75 50	500	T. R.	12
Trowbridge, David	Perry City	Schuyler	42 30	76 55	1,000	N.	1
White, Aaron	Cazenovia	Madison	42 55	75 46	1,260	A.	1
Willis, Oliver R	White Plains	Westchester	41 05	73 40		T.	11
Wilson, Rev. W. D., D. D	Geneva	Ontario	42 53	77 02	567	B. T. R.	12
OHIO.							
Abell, B. F	Welshfield	Geauga	41 23	81 12	1,205	T. R.	12
Alvord, David S	Austinburg	Ashtabula	41 54	80 52	816	B. T. R.	4
Bambach, Dr. G	Ripley	Brown	38 47	83 31	*106	A.	11
Benner, Josiah F	New Lisbon	Columbiana	40 45	80 45	961	B. T. R.	12
Crane, George W	Bethel	Clermont	39 00	84 00	555	T. R.	12
Dole, J. G	Austinburg	Ashtabula	41 54	80 52	816	B. T. R.	3
Engelbrecht, Lud	Portsmouth	Sciota	38 42	82 36	537	B. T. R.	12
Fraser, James B	Saybrook	Ashtabula				T. R.	12
Hammitt, John W	College Hill	Hamilton	39 19	84 26	800	T. R.	11
Harper, George W	Cincinnati	Hamilton	39 06	84 27	*500	A.	11
Haywood, Prof. John	Kingston	Ross	39 29	83 00	692	A.	12
Huntington, George C	Kelley's Island	Erie	41 36	82 42	587	B. T. R.	10
Hyde, Gustavus A. Hyde, Mrs.	Cleveland	Cuyahoga	41 30	81 40	643	B. T. R.	12
Larsh, Thomas J	Eaton	Preble	40 00	74 00	1,400	T.	4
McMillan, Smith B	East Fairfield	Columbiana	40 47	80 44	1,152	A.	12
Mathews, Joseph McD	Hillsborough	Highland				A.	11
Myers, John H	Smithville	Wayne	40 52	81 51	934	T.	3
Newton, Rev. Alfred	Norwalk	Huron	41 15	82 30		T.	12
Phillips, R. C. and J. H	Cincinnati	Hamilton	39 06	84 27	540	B. T. R.	10
Rankin, D. M	Cuyahoga Falls	Summit	42 00	81 00		T. R.	2
Rodgers, A. P	Gallipolis	Gallia	39 00	82 00	600	T. R.	6
Schauber, Hubert A	Centralia	Marion				N.	1
Thompson, Rev. David	Milnersville	Guernsey	40 10	81 45		T.	11
Thompson, Prof. H. A	Westerville	Franklin	40 04	83 00		A.	10
Trembley, J. B., M. D	Toledo	Lucas	41 39	82 32	604	B. T. R.	12

* Above low water in the Ohio river.

List of meteorological stations and observers, &c.—Continued.

Name of observer.	Station.	County.	North latitude.	West longitude.	Height.	Instruments.	No. of months received.
OHIO—Continued.			° ′	° ′	*Feet.*		
Williams, Prof. M. G.	Urbana	Champaign	40 06	83 43	1,015	B. T. R.	12
Wilson, Prof. J. H.	College Hill	Hamilton	39 19	84 25	800	B. T. R.	12
Winchester, E. D.	Austinburg	Ashtabula	41 54	80 52	816	B. T. R.	5
Winger, Martin	Wooster	Wayne	40 49	81 57	872	T.	11
OREGON.							
Hindman, S. M. W.	Auburn	Baker	44 45	118 16	3,350	T. R.	3
Ironside, R. B.	Auburn	Baker	44 37			T.	1
Willis, P. L.	Salem	Marion	44 56	123 01	120	B. T. R.	1
PENNSYLVANIA.							
Bentley, E. T.	Tioga	Tioga				T.	11
Boyers, W. R.	Blairsville	Indiana	40 31	74 43	1,010	T. R.	12
Bruckart, H. G.	Silver Spring	Lancaster	40 05	76 45		T.	11
Brugger, Samuel	Fleming	Centre	40 55	77 53	780	T. R.	12
Clark, Prof. A. G. Aldrich, Truman H.	Westchester	Chester	39 57	75 36		B. T.	3
Eachers, Homer	Philadelphia	Philadelphia	39 57	75 10		B. T.	1
Eggert, John	Berwick	Columbia	41 05	76 15	583	A.	7
Fenton, Elisha	Grampian Hills	Clearfield	41 00	78 40	1,400	B. T. R.	6
Hance, Ebenezer	Morrisville	Bucks	40 12	74 48	30	B. T. R.	12
Heisely, Dr. John	Harrisburg	Dauphin	40 16	76 15		A.	12
Hickok, W. O.	Harrisburg	Dauphin	40 20	76 50	320	A.	1
Hoffer, Dr. Jacob R.	Mount Joy	Lancaster	40 08	76 30		A.	12
Jacobs, Rev. M. Jacobs, H. E.	Gettysburg	Adams	39 49	77 15	624	B. T. R.	6
Kirkpatrick, Prof. Jas. A.	Philadelphia	Philadelphia	39 57	75 10	50	A.	12
Kohler, Edward	North Whitehall	Lehigh	40 40	75 26	250	T.	12
Martindale, Isaac C.	Byberry	Philadelphia	40 05	75 00	70	T. R.	12
Meehan, Thomas Meehan, J.	Germantown	Philadelphia				T.	11
Moyer, H. C.	Williamsport	Lycoming	41 19	77 05	533	B. P. T.	1
Ricksecker, Lucius E.	Nazareth	Northampton	40 43	75 21	530	T.	12
Smith, Wm., D. D.	Cannonsburg	Washington	40 16	80 10	936	B. T. R.	12
Spencer, Miss Anna	Horsham	Montgomery	40 00	75 11	250	B. T. R.	7
Taylor, John	Connellsville	Fayette	40 00	79 36		T.	12
Weeks, James A.	Oil City	Venango				T.	2
RHODE ISLAND.							
Caswell, Prof. Alexis	Providence	Providence	41 49	71 25	120	A.	11
Sheldon, H. C.	Providence	Providence	41 50	71 25		B. T. R.	6
SOUTH CAROLINA.							
Abert, Major James W., U. S. engineers. Suter, Capt. C. R., U. S. engineers.	Hilton Head	Beaufort	32 14	80 40	27	A.	11
Marsh, M. M., M. D. Marsh, Mrs.	Beaufort	Beaufort	32 20	80 46	1	B. T. P.	10
TENNESSEE.							
Blaker, Dr. G. H.	Chattanooga	Harrison				T. P.	1
Stewart, Prof. Wm. M.	Clarksville	Montgomery	36 28	87 13	481	A.	7
UTAH.							
Pearce, Harrison	St. George	Washington	37 00	114 00		T. R.	8
Phelps, W. W.	Salt Lake	Salt Lake	40 45	111 26	4,260	A.	12
Siber, Andrew L.	Vineland	Washington				N.	2
VERMONT.							
Buckland, David	Brandon	Rutland	43 45	73 00		T. R.	6
Cutting, Hiram A.	Lunenburg	Essex	44 28	71 41	1,124	A.	12
Mead, Stephen O.	Rutland	Rutland				T.	5
Paddock, James A.	Craftsbury	Orleans	44 40	72 29	1,100	T. R.	12
Petty, McK.	Burlington	Chittenden	44 27	73 10	367	A.	11
Tobey, James K.	Calais	Washington	44 22	72 09		T. R.	7

List of meteorological stations and observers, &c.—Continued.

Name of observer.	Station.	County.	North latitude.	West longitude.	Height.	Instruments.	No. of months received.
WASHINGTON.			° ′	° ′	Feet.		
Swan, James G.	Neeah Bay		48 41	124 37	40	T.	11
WISCONSIN.							
Breed, J. Everett	Embarass	Waupacca	44 51	88 37		T. R	12
Curtis, W. W	Rocky Run	Columbia	43 26	89 20		T. R.	12
Deckner, Friedrick	Green Bay	Brown	44 29	88 00	732	T. R.	12
Eddy, Levens	Delavan	Walworth	42 39	88 37	957	A.	4
Ellis, Edwin, M. D.	Odanah	Ashland	46 33	91 00	610	T. R.	2
Hicks, J. C.	Lebanon	Waupaca	44 24	88 42		T.	3
Lapham, Incr'se A., LL. D	Milwaukee	Milwaukee	43 03	87 59	593	A.	12
Lups, Jacob	Manitowoc	Manitowoc	44 07	87 45	658	B. T.	12
Mead, H. C.	Waupaca	Waupaca	44 20	89 11	1,000	T.	12
Porter, Henry D.	Beloit	Rock	42 30	89 04	750	B. T. R.	12
Sterling, Prof. John W.	Madison	Dane	43 05	89 25	1,068	A.	6
Wait, M. C	Baraboo	Sauk				T.	2
Whiting, Wm. H.	Genova	Walworth	42 30	89 41		T.	11
Winkler, Carl, M. D.	Milwaukee	Milwaukee	43 03	87 57	600	B. T. R.	12
Woods, William	Weyauwega	Waupaca	44 15	88 50	850	T.	5

Deaths of observers.

John H. Lunemann, S. J., St. Louis, Missouri, 1864.
David Buckland, Brandon, Vermont, July 19, 1864.

Colleges and other institutions from which meteorological registers were received during the year 1864, *included in the preceding list.*

Nova Scotia	Acadia College	Wolfville.
Canada	Magnetic Observatory	Toronto.
Connecticut	Wesleyan University	Middletown.
Illinois	Lombard University	Galesburg.
	University of Chicago	Chicago.
Iowa	Cornell College	Mount Vernon.
	Griswold College	Davenport.
	Iowa State University	Iowa City.
Maryland	Washington College	Chestertown.
Massachusetts	Amherst College	Amherst.
	State Lunatic Hospital	Worcester.
	Williams' College	Williamstown.
Michigan	State Agricultural College	Lansing.
Missouri	St. Louis University	St. Louis.
New York	Institution for Deaf and Dumb	New York.
	Erasmus Hall Academy	Flatbush.
	St. Francis Xavier's College	New York.
	University of Rochester	Rochester.
Ohio	Farmers' College	College Hill.
	Otterbein University	Westerville.
	Urbana University	Urbana
	Woodward High School	Cincinnati.
Oregon	Willamette University	Salem.
Pennsylvania	Central High School	Philadelphia.
	Jefferson College	Cannonsburg.
Rhode Island	Brown University	Providence.
Tennessee	Stewart College	Clarksville.
Vermont	University of Vermont	Burlington.
Wisconsin	Beloit College	Beloit.
	Wisconsin University	Madison.

METEOROLOGICAL MATERIAL CONTRIBUTED IN ADDITION TO THE REGULAR OBSERVATIONS.

Aargauische Naturforschende Gesellschaft.—Witterungs beobachtungen in Aarau, (Switzerland,) 1864.

Bailey, F. J.—See Herschel.

Barrows, N., M. D.—Summary of observations at Sandwich, Massachusetts, for the year 1864.

Barrows, Storrs.—Summary of observations at South Trenton, New York, for the year 1864. Reported by Storrs Barrows, for the Trenton Union Agricultural Society.

Bartlett, Erastus B.—Summary of observations at Palermo, New York, for the year 1864, and a comparison with the preceding eleven years.

Chase, Pliny Earle.—On the Principal Causes of Barometric Fluctuations. By Pliny Earle Chase, M. A., S. P. A. S. (From the Proceedings of the American Philosophical Society.) 8 vo., 8 pp.

Barometric Indications of a Resisting Æther. By Pliny Earle Chase. From the American Journal of Science and Arts, for September, 1864. 8 vo., 8 pp.

Connolly, H.—Observations taken at Fort Nascopic, Esquimaux bay, district of Labrador, from November 1, 1863, to June 30, 1864.

Cook, Eugene B.—Report of Eugene B. Cook, meteorologist of the New York Skating Club, for the season 1863-64. Pamphlet, 30 pp.

Darrell, Judge John Harvey.—Extracts from the Meteorological Report of Observations taken at the Centre Signal Station of Bermuda, in 1860, 1861, 1862, 1863, 1864. Copied from the Royal Gazette.

Extracts from Register kept at the Royal Engineer Meteorological Observatory at St. George's, Bermuda, in 1860, 1861, 1862.

Extracts from the Register of the quantity of rain fallen at Ireland island, Bermuda, in 1860, 1861, 1862, 1863.

[The above were furnished to supply deficiencies in the Smithsonian Record.]

Chart of Bermuda, showing the position of the meteorological stations.

Dreutzer, O. E.—See State Department.

Given, William, United States Vice Consul.—Summary of barometrical and theomometrical observations, and amount of rain, at Fort de France, Martinique, during each month from July, 1863, to June, 1864, inclusive.

Goddard, C. W.—See State Department.

Herschel, Alexander S.—On the detonating meteor of December 5, 1863. By Alexander S. Herschel, esq.; communicated by F. J. Bailey, esq. Read before the Liverpool Literary and Philosophic Society, 8th February, 1864. 8 vo., 8 pp.

Hyde, G. A.—Summary of observations for the year 1864, at Cleveland, Ohio, and a comparison with the preceding eight years.

Ingalsbe, Grenville M.—Observations at South Hartford, Washington county, New York, for the years 1860, 1861, 1862, with summary of maxima, minima, and means.

Ives, William.—Monthly and annual summary of observations, kept for the Young Men's Association at Buffalo, New York, during the year 1864.

Kingston, G. T., M. A.—Mean meteorological results at Toronto, Canada, for the year 1864, compared with the averages of a series of years, by G. T. Kingston, M. A., director of the Provincial Magnetic Observatory.

Lapham, I. A., LL. D.—A table showing the monthly mean temperature of the open air in the shade, at Milwaukee, Wisconsin, from 1837 to 1864, as observed by C. J. Lynde, esq., E. S. Marsh, M. D., Charles Winkler, M. D., and I. A. Lapham, LL. D. Compiled by Dr. Lapham with great care from manuscripts in his possession

Map of Wisconsin, with lines showing the remarkable effect of Lake Michigan, in elevating the mean temperature of January and depressing that of July. By I. A. Lapham, LL. D., Milwaukee, Wisconsin.

Lewis, James, M. D.—Hourly record of temperature at Mohawk, New York, during the year 1864, kept by his metallic self-recording thermometer; also specimens of a register kept by his self-recording barometer.

Logan, Thomas M., M. D.—Contributions to the physics, hygiene, and thermology of the Sacramento river, California. By Thomas M. Logan, M. D. (From the Pacific Medical and Surgical Journal.) 8vo., 8 pp.

Abstract of observations during the year 1864, at Sacramento, California.

Lynde, C. J.—See Lapham.

Marsh, E. S.—See Lapham.

Marsh, Roswell.—Summary of observations at Steubenville, Ohio, from 1831 to 1863, giving the means for each month in every year, the means for the seasons of each year, and the annual means for the whole series.

Morris, Prof. Oran W.—Diagram of temperature and barometer for every day in the year 1863; also, diagram of the monthly quantity of rain in 1860, 1861, 1862, and 1863, at New York.

Murdock, G.—Hints on meteorology, with summaries of observations made at St. John, New Brunswick, between the years 1850 and 1862, the latter included. By G. Murdock, superintendent of water works, St. John. 8vo. 34 pp.

Naturwissenschaftliche Gesellschaft "Isis."—Zustamenstellung der Monats- und Jahresmittel aus den zu Meissen, (Saxony,) 1864, angestellten täglich dreimaligan meteorologischen beobachtungen.

Newton, H. A.—The original accounts of the displays in former times of the November star-shower; together with a determination of the length of its cycle, its annual period, and the probable orbit of the group of bodies around the sun. By H. A. Newton. From the American Journal of Science, May and July, 1864. 8vo., 24 pp.

Abstract of a memoir on shooting-stars, read before the National Academy of Sciences, August 6, 1864. From the American Journal of Science for March, 1865. 8vo., 16 pp.

Paine, H. M., M. D.—Summary of observations for the year 1864, at Clinton, New York.

Phelps, W. E.—See State Department.

Sanger, Henry, (United States consul at Paramaribo.)—Daily means of meteorological elements, at the Georgetown observatory, British Guiana, South America, for December, 1863; published in the Royal Gazette.

State Department, Washington.—Meteorological observations, at Constantinople, from October, 1862, to September, 1863. By C. W. Goddard, consul general at Constantinople.

Daily telegraphic reports of the weather in various places in Europe, communicated to the Central Physical Observatory, St. Petersburg, Russia, for the year ending September 30, 1864. Translated and compiled from the daily issues of the St. Petersburg Vedomost, by William Edwin Phelps, United States consul, St. Petersburg.

Meteorological review for the year 1864, from observations at the Liprocy hospital of Lungeguard, at the city of Bergen, Norway, reduced by O. E. Dreutzer, United States consul, Bergen.

Société Meteorologique de France.—Annuaire de la Société Meteorologique de France, 1864.

Switzerland, Consul General of, at Washington.—Observations at various points in Switzerland, in December, 1863, and January, February, March, and April, 1864. Each month about 50 pages quarto.

Trask, Dr. John B.—A register of the earthquakes in California from 1800 to 1863. Pamphlet, 8 vo., 26 pp.

Trembley, J. B., M. D.—Synopsis of observations for the year 1864, at Toledo, Ohio, and a comparison with the preceding four years.

Tyler, A. Wellington.—Observations from July 26 to October 13, 1864, taken on board the schooner Nelly Baker, on a pleasure voyage from Boston to the coast of Labrador.

Wadsworth, George.—Mean temperature at Hiram, Maine, from 1831 to 1864, inclusive; also, the amount of snow that fell during the same period. The record kept by General Peleg Wadsworth; reductions made by George Wadsworth, civil engineer.

Whitehead, W. A.—Summary of observations at Newark, New Jersey, for the year 1864, and a comparison with the previous twenty years.

Williams, Prof. M. G.—Summary of observations at Urbana, Ohio, for the year 1864, containing the means and extremes of barometer and thermometer, for each month and for the year, together with the monthly amount of rain and cloudiness, and force and direction of the wind.

Winnepissiogee Lake Cotton and Woollen Manufacturing Company.—Depth of rain and melted snow collected in the rain-gauge kept by the company at the outlet of Lake Winnepissiogee, in the town of Laconia, New Hampshire; also, depth of rain and melted snow collected in the gauge at Lake Village, New Hampshire, about four miles south, on the same stream of water, for the year 1864.

Zeigler, C.—Summary of observations for the year 1864, at Du Quoin, Perry county, Illinois.

Unknown.—Results from meteorological observations made at the Royal Observatory, Cape of Good Hope, from January 1, 1842, to January 1, 1863, and a notice of the observations made by La Caille in 1751–'52. 16 pp. folio.

REPORT OF THE EXECUTIVE COMMITTEE.

The Executive Committee respectfully submit to the Board of Regents the following report of the receipts and expenditures of the Smithsonian Institution during the year 1864:

RECEIPTS.

The whole amount of Smithson's bequest deposited in the treasury of the United States is $515,169, from which an annual income of 6 per cent. is derived of		$30,910 14
The extra fund of unexpended income is invested as follows, viz:		
$75,000 in Indiana 5 per cent. bonds, yielding in 1864		3,750 00
$53,500 in Virginia 6 per cent. bonds, yielding in 1864		
$12,000 in Tennessee 6 per cent. bonds, yielding in 1864		
$500 in Georgia 6 per cent. bonds, yielding in 1864		
$100 in Washington 6 per cent. bonds, yielding in 1864		6 00
		34,666 14
Balance in hands of treasurer, January, 1864, and interest due from government		32,353 90
		67,020 04

EXPENDITURES.

For building, furniture, and fixtures	$2,620 77	
For general expenses	14,071 50	
For publications, researches, and lectures	11,907 48	
For library, museum, and gallery of art	8,936 21	
		37,535 96
Balance in treasury and due from government January, 1865		$29,484 08

STATEMENT IN DETAIL OF THE EXPENDITURES OF 1864.

BUILDING.

Building, incidentals	$1,066 32	
Furniture and fixtures in general	804 45	
Furniture and fixtures for museum	750 00	
		$2,620 77

GENERAL EXPENSES.

Meetings of the Board	131 50	
Lighting and heating	1,816 36	
Postage	408 38	
Transportation, general	868 09	
Exchanges	2,753 76	
Stationery	502 77	
General printing	157 76	

Apparatus	102 74	
Laboratory	160 78	
Incidentals, general	631 36	
Extra clerk-hire	599 00	
Salaries, Secretary	3,500 00	
Salaries, chief clerk, bookkeeper, messenger, and laborers	2,439 00	
		14,071 50

PUBLICATIONS, ETC.

Smithsonian Contributions	2,224 57	
Smithsonian Reports	547 00	
Smithsonian Miscellaneous Collections	6,449 06	
Other publications	210 00	
Meteorology	1,339 15	
Researches	125 00	
Lectures	1,012 70	
		11,907 48

LIBRARY AND MUSEUM.

Cost of books and binding	1,953 67	
Assistants in library	1,291 66	
Transportation for library	200 00	
Museum, salary of Assistant Secretary	2,000 00	
Museum, assistants	1,096 56	
Museum, transportation	400 00	
Museum, incidentals	1,080 31	
Explorations	797 76	
Gallery of art	116 25	
		8,936 21
Total expenditure		$37,535 96

From the foregoing it will be seen that the whole income during the year 1864 was $34,666 14, and that the expenditures during the same period were $37,535 96, exhibiting for the *first time*, in the account of the current operations, an excess of the latter over the former of $2,869 82.

According to the statement of the Secretary, the cause of this excess of the expenditure was the constant increase in prices of all the articles used in the operations of the Institution, particularly in printing and paper, and the purchase of gold to defray the expense of the foreign agencies.

To meet contingencies of this kind, however, as well as to carry on all the operations for cash, there had been accumulated in the hands of the treasurer at the beginning of the year the sum of $32,353 90. The unexpended balance, therefore, now in the hands of the treasurer is $29,484 08.

The appropriation by Congress for the preservation of the collections of the exploring and surveying expeditions of the United States has been expended as heretofore, under the direction of the Secretary of the Interior, in assisting to pay the expenses of extra assistants in the museum, and the cost of arranging and preserving the specimens. The articles intrusted to the care of the Institution by government are in good condition, and the distribution of the duplicate specimens belonging to government, as well as those of the Institution, has been industriously prosecuted during the year.

From the examination made by the committee it appears that the affairs of the Institution are in a prosperous condition; that all the operations have been

continued with unabated energy; that notwithstanding the depreciation of the value of the income, the expenditures have but little exceeded the current receipts, and that provision had been made even for this contingency by the previous accumulations in the hands of the treasurer.

The Executive Committee are informed by the Secretary that the remainder of the legacy of Smithson, amounting to about $26,000, has been received in coin, and deposited with the Treasurer of the United States.

In conclusion, it may be stated that the whole amount of the Smithsonian fund, including the original legacy and the additions which have since been made to it, together with the balance in the hands of the treasurer, and the State stocks estimated at their present market value, amounts to about $690,000.

The committee agree with the Secretary in opinion that, as far as possible, the active operations of the Institution should be continued, and the curtailments rendered necessary by the depreciation of the currency be made in expenditures for those objects which can most readily be postponed. For the year 1865 the same estimates are submitted as those for 1864, with such diminution as the Secretary may deem it advisable to make.

The committee have carefully examined the accounts of the Institution and the books as posted by Mr. Randolph for the past year, and find them to be correct.

Respectfully submitted.

RICHARD WALLACH, *Chairman*.

FEBRUARY, 1865.

JOURNAL OF PROCEEDINGS

OF

THE BOARD OF REGENTS.

WASHINGTON, *January* 9, 1865.

A special meeting of the Board of Regents was held this day at 7½ o'clock p. m. in the hall of the Institution. Present: Hon. H. Hamlin, Hon. S. P. Chase, Hon. L. Trumbull, Hon. G. Davis, Hon. S. S. Cox, Hon. J. W. Patterson, Professor L. Agassiz, and the Secretary, Professor Henry.

Mr. Hamlin was called to the chair.

The Secretary stated that this meeting had been called in accordance with a provision of the law of Congress authorizing a meeting at any time, at the request of three members of the Board. That the objects of this meeting were—

First. To announce officially the death of Chief Justice Taney and General Totten, both members of the Board from the beginning of the Institution, and who had ever evinced a lively interest in its prosperity, and had faithfully discharged their duties as guardians of the trust.

Second. To elect a Chancellor or President of the Board in place of Chief Justice Taney.

Third. To consider the disposition to be made of the remainder of the legacy of Smithson, which was now deposited with Messrs. Peabody & Co., of London, subject to the order of the Institution; and,

Fourth. To consider the report of the committee appointed at the last session of the Board, relative to the suggestions of Professor Agassiz as to the separate maintenance of the museum, &c.

On motion of Mr. Cox, it was resolved that the proper expression of sympathy be tendered to the families of the Regents whose deaths have been announced, and that provision be made for the preparation of an account of their lives and labors for the annual report to Congress.

On motion of Mr. Cox, Chief Justice Salmon P. Chase was unanimously elected Chancellor of the Institution.

On motion of Mr. Chase, the Secretary was instructed to draw the money now in England, and to deposit it with the Treasurer of the United States.

Professor Agassiz, as chairman of the special committee, appointed at the meeting held March 15, 1864, to report suggestions for extending the active operations of the Institution, and for the separate maintenance of the collections, at the expense of the government, submitted a report.*

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* This report was lost in the fire, and the absence of Professor Agassiz from the country has rendered it impossible to obtain another copy in time for insertion in this journal.

The opinion was expressed by several members of the Board that the views of Professor Agassiz were highly important, and believed to be such as were entertained generally by the scientific men of the country, but in consideration of the financial condition of the government, the present time was not favorable for action in regard to them.

On motion of Mr. Trumbull, the consideration of the subject was postponed to the annual session to be held in January, 1866.

The Secretary stated that the question had arisen at a previous meeting of the Board as to whether the interest on the Smithsonian fund, permanently in the treasury of the United States, ought not to be paid in coin, in common with the interest on other trust funds in charge of the government; that he had addressed a letter to the Secretary of the Treasury on this subject, but on account of the large demands on the government for the prosecution of the war, he had not pressed a decision of the question.

On motion of Mr. Chase, it was

Resolved, That the Secretary be instructed to renew the application to the Treasury Department, in behalf of the Board, for the payment of the interest in coin.

The meeting then adjourned.

WASHINGTON, *January* 19, 1865.

In accordance with a resolution of the Board of Regents of the Smithsonian Institution, fixing the time of beginning of their annual session on the third Wednesday of January in each year, a meeting was called for this day.

No quorum being present, and the Secretary having stated that the book-keeper had not yet been able to make up the annual accounts, the Board adjourned, to meet at the call of the Secretary.

WASHINGTON, *January* 28, 1865.

A meeting of the Board of Regents was held at 3 o'clock p. m. in the east wing of the Smithsonian building. Present: Hon. S. P. Chase, Hon. H. Hamlin, Hon. L. Trumbull, Hon. J. W. Patterson, Hon. R. Wallach, Mr. Seaton, treasurer, and Professor Henry, Secretary.

The Chancellor, Chief Justice Chase, took the chair.

The Secretary stated that the principal object of this meeting was to officially inform the Regents that, on the afternoon of Tuesday, January 24, a fire broke out in the roof of the main building of the Smithsonian Institution, which destroyed the principal part of the contents of the rooms in the upper story of the building and the adjoining towers. The loss, however, did not include the large library, the museum, with the government collections and those of the Institution, the duplicate specimens intended for distribution, and the meteorological records. The accident would not, therefore, materially affect the essential operations of the Institution, which would be continued as usual.

The Secretary stated that, immediately after the occurrence of the accident, he had applied to the Secretary of War, Mr. Stanton, for aid in constructing a temporary roof to protect the building and its contents from the weather. The Secretary of War expressed his willingness to grant this, provided the President gave his sanction, and the expense should be refunded to the department. The latter was promised on the part of the Institution by the Secretary, after consultation with the Chancellor. The President readily gave his consent to the proposition, and General Rucker, of the Quartermaster's Department, furnished the materials, and detailed a large force of carpenters and laborers, under the direction of Mr. E. Clark, to erect a temporary roof, which would be sufficient to protect the building from storms, and would not interfere with the construction of a permanent covering.

At the suggestion of the Chancellor, it was

Resolved, That the measures which had been taken by the Secretary be approved.

Mr. Patterson informed the Board that the House of Representatives had adopted, on the motion of Hon. Mr. Rice, a resolution directing the Committee on Public Buildings and Grounds to inquire into the origin of the fire, the approximate loss to the government and private persons, the means necessary to preserve the remaining portions, &c.

The Chancellor remarked that it would be proper that a joint committee should be appointed, to be composed of members of the Senate, of the House of Representatives, and of this Board, to take the whole subject into consideration.

In anticipation of this, however, it was thought advisable that a special committee should be appointed to report directly to the Board; and, on motion of Mr. Wallach, it was

Resolved, That a committee be appointed to inquire into the origin of the fire. to ascertain the extent and character of the loss sustained, and to make suggestions as to what measures should be adopted for the repair and improvement of the building.

The Chancellor appointed the mover of the resolution Mr. Wallach, and the Secretary, as the committee.

The Board having examined the building, adjourned, to meet on Thursday evening at 7½ o'clock p. m.

WASHINGTON, *February* 2, 1865.

A meeting of the Board of Regents of the Smithsonian Institution was held at 8 o'clock p. m. at the residence of one of the Regents, Hon. R. Wallach, Mayor of Washington. Present: Hon. H. Hamlin, Hon. G. Davis, Hon. J. W. Patterson, Hon. S. S. Cox, Hon. R. Wallach, and the Secretary, Professor Henry, and, by invitation, Hon. J. H. Rice, chairman of the Committee on Public Buildings of the House of Representatives.

Mr Hamlin was called to the chair.

The minutes of the meetings held on the 9th, 19th, and 28th of January were read and approved.

Mr. Wallach presented the report of the Executive Committee for the year 1864, which was read and adopted.

The Secretary stated that, in accordance with the instructions of the Board, he had renewed the inquiry to the Secretary of the Treasury whether the interest of the Smithsonian fund ought not to be paid in coin or its equivalent, but had not yet received a reply, it having been referred to the Solicitor of the Treasury for a legal opinion.

On motion of Mr. Davis, it was

Resolved, That if the Secretary of the Institution should ascertain that the legal opinion of the Solicitor would be adverse to the application, that he should request the Secretary of the Treasury to submit the question to Congress for its action.

Professor Henry presented the question as to the disposition of the residuary legacy of Smithson which had been received from England, and was now on deposit with the Treasurer of the United States.

On motion of Mr. Patterson, it was

Resolved, That the Secretary be instructed to invest the money now on deposit with the Treasurer of the United States, derived from the residuary legacy of James Smithson, in United States bonds bearing $7\frac{3}{10}$ per cent. interest.

Mr. Wallach presented the following report from the special committee appointed at the last meeting to inquire into the origin of the fire, &c., which was read and adopted:

REPORT OF THE SPECIAL COMMITTEE OF THE BOARD OF REGENTS OF THE SMITHSONIAN INSTITUTION RELATIVE TO THE FIRE.

The special committee appointed by the Board at its meeting on January 28, 1865, to inquire into the origin of the fire at the Smithsonian Institution, to ascertain the extent and character of the loss sustained, and to make suggestions as to what measures should be adopted for the repair and improvement of the building, respectfully report that they have performed the duty assigned them, so far as the time and their means of information would permit.

I.—THE ORIGIN OF THE FIRE.

The testimony has been taken of all persons connected with the establishment that had any knowledge of the occurrence, and a written account of the whole is herewith submitted; also a report from Colonel B. S. Alexander, United States army, who superintended the fire-proofing of the main building, of his examination of the flues connected with the accident.

It is evident, from the concurrent testimony thus obtained, that the fire commenced in the southwest part of the roof of the main building in the woodwork immediately under the slate covering, and that it was kindled by the heated air or sparks from a stove which had been temporarily placed in the room immediately below. The pipe of this stove had been inserted, by mistake, into a brick furring-space resembling a flue, which opened under the rafters instead of into the chimney flue, within a few inches of the latter. By whom the hole into which the pipe was inserted was originally made is not known, but it is remembered that a stove-pipe was put into it as far back as 1854, at the time of the exhibition held by the Mechanics' Institute in the building. No

fire, however, had been in this room for ten years previous to Monday, 15th January, when the mechanist and carpenter of the Institution were engaged, with several other of the employés, in rearranging the pictures of the gallery, the weather at the time being unusually cold. These persons, for temporary convenience, set up the stove above mentioned, intending to remove it as soon as their task was finished. A coal fire, kindled with wood, had been burning in this stove for eight days previous to the conflagration, yet it appears from the testimony that no evidence of combustion was observed by a person who passed through the loft six hours before the breaking out of the flames. It is probable, however, that the wood had been undergoing a process of charring for several days.

On account of the very expensive style of architecture selected for the building, and the limited means at the command of the Board, the plan had been at first adopted of finishing the interior of the whole edifice with wood and plaster. A large portion, however, of the interior woodwork of the main building, after the roof and exterior had been finished, gave way and fell; whereupon the Regents ordered the removal of the woodwork and its place to be supplied with incombustible materials. Thus the main building was rendered fire-proof, with the exception of the supports of the roof, which being covered with slate was assumed to be safe. The only danger of the occurrence of fire was supposed to exist in the two wings and the towers, and to guard against this contingency especial precautions were constantly observed, viz: 1. No smoking was allowed in any part of the building at any time. 2. No lights were allowed to be carried from one part of the building to another except in lanterns. 3. Three coils of large hose were deposited, ready for use, one in the upper story and the other two on the first floor of the building; and there were water-pipes in the basement with faucets. 4. Barrels and buckets, kept constantly filled with water, were placed at different points of the building. 5. The rule was observed of cleaning the flues every autumn before the commencement of fires. 6. A watchman was employed each night, who made every hour the rounds of all the rooms in the building, giving special attention to those in which fire had been kindled during the day, including the apartments occupied by the family of the Secretary.

These precautions, however, as it has proved, were insufficient—the fire having occurred at a point where no danger was apprehended, and to which access could with difficulty be obtained.

II.—THE CHARACTER AND EXTENT OF THE LOSS SUSTAINED.

The loss to the Institution was as follows:

1. The contents of the Secretary's office, consisting of the official, scientific, and miscellaneous correspondence, embracing 35,000 pages of copied letters which had been sent, at least 30,000 of which were the composition of the Secretary, and 50,000 pages of letters received by the Institution. Here, moreover, were lost the receipts for publications and specimens; reports on various subjects which have been referred to the Institution; the records of experiments instituted by the Secretary for the government; four manuscripts of original investigations, which had been adopted by the Institution for publication; the manuscript material of the report of the Secretary for 1864; a large number of papers and scientific notes of the Secretary; a series of diaries and memorandum books, and a duplicate set of account books, prepared during the last twelve years, with great labor, by Mr. Rhees, the chief clerk; also, about one hundred volumes of valuable works kept at hand for constant reference.

2. In the apparatus room, the large collection of scientific instruments, including the donation of the late Dr. Hare.

3. A part of the contents of the Regents' room, including the personal effects of Smithson, with the exception of his portrait and library.

4. The contents of the rooms in the towers, including the meteorological instruments, the workshop, containing a lathe and a large number of valuable tools, nearly all the stock on hand of the duplicate copies of the annual reports, and many other public documents and books intended for distribution to libraries, as well as a quantity of stationery, hardware, &c.

5. The wood-cuts of the illustrations contained in the Smithsonian publications.

The loss to other parties was as follows:

1st. The contents of what was called the Picture Gallery, viz: *a*. About two hundred portraits, nearly all of life size, painted and principally owned by Mr. J. M. Stanley, formerly of this city, and now of Detroit, Michigan, and which were on deposit in the institution. *b*. A number of half-size Indian portraits, painted by Mr. King for the government. *c* A copy, in Carrera marble, of the antique statue known as the "Dying Gladiator," by John Gott, and owned by Mr. J. C. McGuire, of this city.

2. A number of surveying instruments belonging to the government.

3. The clothing, books, and private effects of several of the persons connected with the Institution, and of those engaged in scientific studies.

4. The library removed from Beaufort, South Carolina, by the army, and also that of Bishop Johns, from Fairfax Theological Seminary, given in charge to the Institution by the Secretary of War for safe-keeping, which libraries were stored in an upper room in the south tower.

Independent of injury to the building, the loss to the Institution, as far as it may be estimated and can be restored by money, may be stated at about $20,000, and to individuals $26,000, viz: To Mr. J. M. Stanley, $20,000; Mr. J. C. McGuire, $1,000; Professor Joseph Henry, $1,500; Mr. W. J. Rhees, $1,200; Mr. W. DeBeust, $1,300; and all others, $1,000.

Although the loss which the Institution and individuals have sustained is much to be regretted, yet it is a source of consolation that by far the greater part of the valuable contents of the building have escaped without injury. The valuable library of the Institution, the most extensive, in regard to the transactions of learned societies and scientific books, in this country; the museum, including the collection of the exploring expedition and those of the Institution; the large stock of many thousand duplicate specimens for distribution to all parts of the world; the records of the museum; a large portion of the correspondence relative to natural history; nearly all the records of meteorological observations which have been accumulated during the last fifteen years; the sets of Smithsonian publications (except the annual reports) which have been reserved to supply new institutions, and the stereotype plates of all the works which have been published during the last four or five years, have been saved. All the original vouchers of the payments made by the Institution, the ledger in which they were posted, and the daybook from 1858, were also preserved, having been deposited in a safe in the Regents' room. The contents of the connecting range between the library and the museum are uninjured; this includes a series of plaster casts and portraits of distinguished men, among the latter a life-size portrait of Guizot, by Healy; an original full-length figure of Washington, by the elder Peale; and also a valuable series of rare engravings illustrative of the history of the art, purchased from the Hon. George P. Marsh.

All the important acts of the Regents from the beginning, and an account of the operations of the Institution, having been published from year to year in the several reports to Congress, a continued record of the history of the establishment from the beginning is, therefore, still in existence. As these reports have been widely distributed, they are generally accessible to the public.

The burning of the roof of the building can scarcely in itself be considered

last summer, leaving a family of daughters in a destitute condition; that he had given the widow permission to sell articles of refreshment, exclusive of intoxicating liquors, to the visitors of the museum, and that he thought, in consideration of the long and faithful service of the deceased, the expenses of his funeral should be paid from the Smithsonian fund.

On motion of Mr. Wallach, it was

Resolved, That the Secretary be authorized to pay the funeral expenses of John Connor.

On motion of Mr. Patterson, the vacancy in the Executive Committee was filled by the election of General Delafield.

On motion of Mr. Trumbull, it was

Resolved, That the annual salary of the Secretary of the Institution be increased one thousand dollars, that of the Assistant Secretary five hundred dollars, and of the Chief Clerk three hundred and fifty dollars, and that the said increase take effect from the 1st of January, 1865.

The Secretary stated what measures had been taken in regard to the preservation of the Smithsonian building, and that Mr. A. Cluss, architect, had been employed to prepare plans and estimates.

On motion of Mr. Cox, it was

Resolved, That the whole subject of the repairs and reconstruction of the building, and the disposal of the stocks held by the Institution, be intrusted to the Chancellor, Secretary, and the Executive Committee.

The Board then adjourned *sine die*.

GENERAL APPENDIX

TO THE

REPORT FOR 1864.

The object of this appendix is to illustrate the operations of the Institution by reports of lectures and extracts from correspondence, as well as to furnish information of a character suited especially to the meteorological observers and other persons interested in the promotion of knowledge.

MEMOIR OF DELAMBRE.

BY JOSEPH FOURIER, PERPETUAL SECRETARY OF THE FRENCH ACADEMY OF SCIENCES.

TRANSLATED FOR THE SMITHSONIAN INSTITUTION BY C. A. ALEXANDER.

To have acquired, from his early youth, a familiar knowledge of the great works of antiquity, and of the languages and literature of modern times; to have dedicated himself to the study of the heavens, and identified his name with an enterprise of wide renown and eminent utility; to have written with ability the history of ancient science, as well as of the most recent discoveries; and with these proofs of mental superiority to have united the noblest qualities of the heart—in these sentences may be summed up the entire life of the distinguished man whose character and labors I propôse to retrace. Few will fail, in the traits thus given, to recognize Delambre, or to feel in the review a renewal of the regrets occasioned by his recent loss.

Delambre was born at Amiens, the 19th of September, 1749. The abbé Delille, a distinguished professor of letters in the college of that city, soon recognized in his youthful pupil a union of the gentlest manners with surprising powers of memory and an early familiarity with the ancient languages. In developing these first germs of talent and taste, the abbé succeeded in inspiring a passion for continued and unremitting application, the indispensable prerequisite of all permanent success. A friendship was thus cemented between these two celebrated individuals which continued to the end, with equal disinterestedness and constancy on the part of both.

To continue in the capital a course of study thus auspiciously commenced was beyond the means of Delambre's family, already burdened with a numerous charge. Fortunately, however, a gratuitous scholarship had been founded at some former period by one of the members of that family, in the University of Paris, and being at the disposal of the city of Amiens, was now conferred on the young Delambre. The benefaction thus reverted towards its source, nor could a destination more just or worthy have been given it. The recipient, already distinguished in every path of literary study, was destined one day to reflect on his country the lustre of imperishable labors. While completing the course of studies which bears the name of philosophy, he still recurred, with unwearied assiduity, to that ancient literature which had shaped the culture of his earlier years, and which the pupil of the abbé Delille could scarcely fail to prosecute with brilliant success.

The time allotted to the appointment he held had passed away, and his family, seemingly persuaded that talent ought to suffice for everything, left him to provide for his own establishment. More than a year thus elapsed in vague expectation, during which time the most extraordinary privations were endured, not merely with constancy, but indifference. It would be difficult to believe, had we not Delambre's own word for it, to what an extent this restriction of his expenses was carried. Absorbed in literary and historical studies, he scarcely regarded as desirable what others would have considered indispensable. Silently laying the foundation of his future labors, he engaged at this time in extensive translations from the Latin, Greek, Italian, and English, and commenced also the study of mathematics; the whole not with any view to profit,

though he might easily have obtained it, but with the sole purpose of prefecting his own knowledge.

Living thus apart and unnoticed, yet free and happy, he knew or indulged no passion but that for study. His time, the only property he could call his own, was secured to him; no importunate visitor appropriated his leisure; his talents gathered strength and expansion daily for the future service of astronomy and letters. Solitude may thus become an inspiration for genius; it exalts the thoughts, dissipates the desire for sudden and vulgar notoriety, and prepares the way for works worthy to be the admiration of after ages.

The extraordinary merit of Delambre, the habitual mildness of his character, the determination with which he applied himself to a revisal of his whole previous course of study, at length attracted attention. As he had been advised to give some years to teaching, he repaired with this purpose to Compiègne, but remained there only a short time. The resources of the capital had become indispensab e to his plan of study, and returning to Paris, he entered on the same course of life as before; this time, indeed, with some additional advantages which he could no longer decline with prudence, and which soon secured him a settled and independent position. It was now that Delambre first felt himself irresistibly impelled to a scientific career. Without neglecting literature and history, he explored the theories of mathematics, and applied himself assiduously to the study of astronomy and physics. A fidelity to his purpose formed at all times a distinctive characteristic of his genius. No one ever threw into his pursuits more of the spirit of sequence, or traversed with more constancy the vast field of human attainments.

When he presented himself at the college of France to hear the lessons of Lalande, he had already studied the works of that astronomer, and had even written commentaries upon them. Here he first attracted notice by reciting, on an occasion which casually offered itself, an entire passage from the Greek poet Aratus, which he illustrated by the criticisms of various annotators. Lalande naturally felt curious to know in what manner his own treatise of astronomy had been annotated by so accomplished a student, nor could he long remain in doubt as to the value of this new acquisition to the interests of science. From that time he regarded Delambre as a fellow-laborer; dissuaded him from a useless attendance on the public lessons, and associating him with his own private labors, confided to him the most difficult astronomical calculations. He induced Mon. Dassy, whose son had previously received lessons from Delambre, to establish a private observatory at his hotel. Here, being provided with the necessary instruments, Delambre entered on a course of the most extensive observation and research; digested a plan for reforming all the astronomical tables, and may be said to have consecrated his life to the study and description of the heavens. This destination had presented itself to his mind at Compiègne, and was originally suggested by a physician of that place—a man of learning, who was the confidant of Delambre's studies, and had remarked his extraordinary turn for earnest and continuous occupation. Lalande, who knew no distinction between his own interests and those of the science he cultivated, sought the acquaintance of the individual who had given this judicious advice, and thanked him in terms to which no personal obligation could have imparted greater warmth.*

It remains now to indicate the important researches to which Delambre applied himself, and which constituted his title to a place in the Academy of Sciences. Herschel, about this time, had observed on the extreme confines of the planetary world an orb till then unknown—a brilliant discovery, which afforded a new proof of the truth of the modern theories of physics. This planet was found to

* Lalande, in allusion to his own part in directing the studies of Delambre, was accustomed boastingly to style the latter his *best work*.—TRANSLATOR.

be subjected to the mathematical laws of gravitation; its course and the places which it had occupied in the heavens were susceptible of demonstration. It was thus ascertained that it had been before observed by different astronomers, who had not, however, distinguished it from the fixed stars. Delambre undertook the preparation of tables respecting its movements, and published them without delay, presenting with great exactness all the observations which had been made up to that time. The Academy of Sciences had proposed this subject for one of its annual prizes, and bestowed its suffrage on the work of Delambre. A striking attestation was herein afforded to the precision of the new astronomical methods; for though Herschel's planet had described but a tenth part of its course since its discovery, its movements were determined with as much exactness as those of other planets, our knowledge of which ascends to periods of remote antiquity.

To Delambre we owe also those tables of the sun which were published at the same time; as likewise those of Jupiter and Saturn. He undertook further to construct ecliptic tables of the satellites of Jupiter, and completed this arduous and immense labor in a few years.

The object of astronomical tables is to represent the actual state of the heavens at a given moment. They proceed upon the general principle of the stability of natural laws, and from the past enable us to attain a knowledge of the future. These researches are guided by geometry, which was said by Plato to have its abode in the heavens. They are controlled also by other mathematical theories which the moderns have invented, and which have served to detect the causes and the laws of celestial movements.

The most general phenomena, it was first perceived, were necessary consequences of the mathematical laws of gravitation; still later, more precise observations indicated irregularities in the course of certain heavenly bodies, which did not appear to result from the general laws. It was asked if the resistance of an etherial medium might not affect the planetary movements; if gravitation was as simple in its action as had been supposed; if the transmission of its force was instantaneous or progressive, like that of light. These doubts no longer exist, and it was in the bosom of this Academy that they were resolved. The inequalities which seemed inexplicable are necessary results of a mutual action between the celestial bodies; they are no exceptions to the mathematical laws of gravity, which, on the contrary, they serve to confirm. The planetary world oscillates between limits which it cannot transcend; it contains within itself principles of stability and duration which suffice to govern and preserve it. It was the imperfection of knowledge alone which led astronomers to have recourse to occasional compensatory causes. The more the universe has been studied, the more admirable have appeared the unity and simplicity of its laws. Never have the sciences attained perfection without evincing the immutable order which an infinite wisdom has impressed on all its works.

All these momentuous questions respecting the system of the world were under discussion when Delambre gave himself with characteristic ardor to the study of astronomy. He assisted at the sitting of the Academy when Laplace communicated his important discoveries on the respective inequalities of Jupiter and Saturn, and at once conceived the design of applying the results of that profound analysis to perfect the tables of those two planets.

To collect and discuss all the observations which have been taken, so as to render them comparable with one another and with the results of scientific theory; to distinguish, by this means, the elements proper to be employed, and to assign to those elements the value due to them in reconciling the theorems of analysis with the observed facts; such is, in general, the succession of steps to be followed in the construction of astronomical tables.

Delambre applied himself especially to those of the satellites of Jupiter—an undertaking so arduous, and of such vast extent, that nothing less than the two

powerful motives of public utility and the grandeur of the subject could have sustained him in its prosecution. The satellites which accompany Jupiter, and which disappear when they enter into his shadow, were the first celestial bodies revealed to us by the telescope. Their eclipses, so entirely analogous to those of the moon, recur much more frequently than the latter; for a single one of these bodies is obscured four times in the space of seven days. Galileo, who first contemplated these singular phenomena, quickly inferred that observations of this kind might be rendered eminently subservient to the promotion of geographical science. In fact, it required but a knowledge of the courses of these satellites, and their reduction into tables of sufficient exactness, to rectify a multitude of enormous errors in the determination of longitude, especially in regard to the eastern portions of the Old Continent. It is true, indeed, that many causes concur to limit the use and precision of this method, but it cannot the less be looked upon as an invaluable source of discovery, and in its consequences as one of the most fortunate of modern inventions; for even at this day the navigator derives the easiest means of ascertaining the proximity of land and the relative position of the places he is approaching from a group of celestial objects, whose minute size seemed to withdraw them forever from human observation. Nor is this the only consideration by which this world of Jupiter is recommended to our admiration, and invested with an interest not inferior to that of any other portion of the heavens. To an attentive observation of the eclipses of his satellites we owe our knowledge of the fact that the action of light is not instantaneous; and from the same source has been derived the precise measure of the time which is required for its propagation from the sun to our globe—a capital discovery, made by Reaumer at the observatory of Paris, and which a subsequent theory has fully confirmed.

The system composed of Jupiter and his four satellites is a world apart, whose rapid revolutions mirror to us those which are taking place in the general system of the sun and planets. Hence, the study of the inequalities of those satellites may be said to economize astronomic time, as they present to our view a class of phenomena which will require an immense series of ages for their development in the planetary system.

The three first satellites of Jupiter are subjected, by their mutual action and that of the planet, to two very remarkable laws, not less simple or constant than those of Kepler. There is a reciprocal dependence between their movements and their positions, so that the place of two of them being known, that of the third is thereby determined; and as one consequence of this state of things, they can never be all three eclipsed at the same time. Laplace had discovered these laws, and had demonstrated that they are necessary results from the mutual action of the satellites, and that the same cause tends to perpetuate their operation. All observation affording subsistent proof of the truth of these laws. Delambre made those admirable theorems of Laplace the basis of his researches. He had occupied several years in the composition of ecliptic tables founded upon them, when the Academy of Sciences proposed the same inquiries as the subject of a prize. This was awarded to Delambre. As he had before obtained this distinction on account of the planet Herschel, he now a second time earned it on account of those of Galileo. A short time before this he had been elected a member of the Academy.

About this time the execution of a grand and difficult project was set on foot in France—a project whose importance had attracted the interest of all enlightened nations, its object being to establish a uniform system of measures founded on some natural and invariable base. As the chief element of the French metrical system, a determinate portion of the terrestrial meridian was fixed upon, and a fortunate opportunity was thus offered of renewing those important geodesic operations which have carried to probably the highest attainable degree of precision our knowledge of the figure and dimensions of the

globe. To Delambre and Mechain was confided the task of measuring an arc of the meridian from Dunkirk as far as Barcelona—a vast undertaking, whose character, progress, and difficulties it is impossible here to detail. Suffice it to say, that its success was principally due to Delambre; he has written its history, and it is to his works we must resort if we would acquire a just and accurate knowledge of the care which it exacted and the results which it produced.

To trace this meridional line of more than two hundred leagues in length, and actually to measure it in its whole extent, is what could never have been proposed: it is the office of geometry to supply the place of immediate measurement. But what a multitude of obstacles does the execution present! The temperature of the air and of solid bodies is continually changing; the atmosphere, whose state is so variable, deflects light from a direct course; the unequal height of the points to be observed, the difficulty of selecting, placing, and maintaining signals, all conspire against the exactness of results, which may be varied also by the attraction of mountains and influenced by inequalities in the figure or mass of the globe. Physics, astronomy, and mathematical analysis must combine their lights to dissipate these causes of uncertainty and error, and to enable the operator to distinguish among the various species of proof that which is applicable to his immediate object.

We must restrict ourselves here to citing one remarkable instance of verification which occurred in comparing the two bases of Perpignan and Melun, each in extent about three leagues. Delambre had taken the length of both by actual measurement. Now, one only of these measurements was necessary, for the two bases being comprised in a common chain of consecutive triangles, the one could be deduced from the other by calculation. Having submitted the operations to this singular proof, the more decisive from the fact that the two bases are about two hundred and twenty leagues distant from one another, it was found that there was not the difference of the third of a metre between the result of the calculation and that of the measurement. Thus was determined, by a trigonometrical operation, a line of about three leagues in length from a distance of more than two hundred leagues, and the error was less than a foot, that is to say, the thirty-sixth thousandth part of the line calculated. I will not say that Delambre was surprised at this coincidence; but he was at least highly gratified, for it was the result alike of his own assiduity and of the astonishing accuracy of the instruments. Those used for the measurement of angles, as well as for taking astronomical observations, were the repeating circles of Borda, whose advantage especially consists in the distribution of any one error over a multitude of observations. The process adopted for measuring the bases, which is likewise due to the same great physicist, consists in the right application of a measure formed of platina, which serves at the same time as a measure and a thermometer.

Thus far we have spoken only of difficulties incident to geodesic operations in general, but the conductors of the grand work we are considering were often impeded by obstacles of a different nature. Their progress was beset not only with keen anxieties, but injurious suspicions and personal annoyance. Mechain, who has left us a great number of highly valuable observations, was subjected to quite a long imprisonment, and suffered much from the effects of unremitting fatigue. The principal operation, however, was finished, and, as he had always designed, Mechain wished to carry the measurement as far as the Balearic islands, but died in a foreign land before the results of his painful labors were laid before the public. Delambre, to whose share had fallen a much more extensive portion of the work, collected all the elements of it, and published them in a leading work which must be regarded as one of the noblest monuments of science. He had quitted Paris in the last days of the month of June, 1792. It is easy to judge how little favorable the political condition of France

at the time, and the violent passions which inflamed all the parties of the realm, must have been to a scientific enterprise whose advantages could only be appreciated by enlightened minds.

Suspicions accordingly were soon excited by the arrangements which it was necessary to make, especially during the night, and by the employment of incomprehensible signals and instruments. The villagers flocked together; they questioned the astronomers, and demanded the instructions under which they acted, and which seemed to those ignorant minds the cover to some guilty mystery. Delambre, who always believed that good faith, patience, and the desire of being useful, would triumph over all obstacles, showed the instruments, explained their use, and, to employ his own expressions, undertook to give lessons in geodesic astronomy on the public squares of Lagny, Epinay, and St. Denis. By this means he succeeded in convincing some of his auditors. But the renewal of these annoying and perilous interruptions made a suspension of the work unavoidable. And although this first obstruction was not of any long duration, and Delambre was allowed after a while to prosecute his original operations, yet he was called shortly after to endure a persecution still more odious and protracted. Under the most frivolous pretexts he was excluded from the commission for determining the new system of measures. The record of this decision may still be consulted, and we there see that it was the moderation of his opinions, which was imputed to him as a crime, and which led to his being forbidden even to take part in the measurement of the meridian. The same order excludes Borda, Delambre, Coulomb, Laplace, and Lavoisier; it bears the signatures of Robespierre, Billaud-Varenne, Couthon, and Collot-d'-Herbois. How significant this apposition of names, the most illustrious and the most odious. But I shall not detain your attention on those unhappy times which are already far removed from us; the history of science delights only in the recollections of a state of public harmony and concord.

Delambre might well entertain disquieting apprehensions, and he accordingly did all in his power to cause himself to be forgotten. Restored to his sedentary occupations, he divided his time between science and letters. The Muses a second time embellished his retreat; they had animated his youth; they consoled his manhood. The Muses are beneficent and hospitable; they offer an asylum to every misfortune; they welcome outraged merit and lift it above the injustice of cotemporaries; they smile upon those who have no patrimony but their time and fill their solitude with charms; to all conditions of life they whisper consoling hopes and inspire noble sentiments.

After an interruption of two years, Delambre, who had carefully preserved all the results of his labors, found it practicable to renew his exclusive attention to them. He resumed them at first under a different title, but after no long time arrangements for continuing the measurement of the meridian were fully restored. With his usual constancy, Delambre prosecuted all the details of the vast undertaking, which was completed before the last year of the century. The results obtained were calculated according to different methods, several of which were proposed by Delambre. A remarkable theorem of Legendre's was also employed in these calculations and evinced its peculiar adaptation to the uses of geodesic mensuration,

When the importance of the subject is considered, the questions of astronomy, geometry, and physics, which it was necessary to resolve, the celebrated names of French or foreign savants, who lent their co-operation to the inquiry, and the weighty and durable consequences of its result, it is not too much to say that no other application of science is to be compared with this as regards its character of exactness, utility and magnitude. Such was the judgment passed upon it by all the academies of Europe, and the opinion of the Institute of France was formally expressed when called upon to designate the most important application of mathematical or physical science which had occurred within

ten years. With entire unanimity of suffrages the prize was assigned to the author of the base of the metrical system.

The length of a determinate part of the meridian was at last known with extraordinary precision, and upon this result, together with that of the memorable experiments of Lefevre-Gineau, has been founded the established system of French measures.

This great work comprehends all the experiments which have been made in different places on the length of the pendulum conformably to the processes invented by Borda; as well as all the observations made with the design of prolonging the arc of the meridian to Fromentera, and also towards the north, where it unites the geodesic labors of France with those of Great Britain.

One of the most remarkable results of modern science is that which relates to the elliptical form of the terrestrial globe. The flattening of the regions around the poles, as determined by the revolution of the globe upon its axis, is demonstrated by all our geodesic measurements, as it is also by a comparison of the lengths of the pendulum; and, what must be regarded as one of the most astonishing attestations to the perfection of our astronomic theories, the measure of this ellipticity is deducible with great exactness from an attentive observation of the movements of the moon. In the course of that planet, irregularities have been discovered which are owing to the action of the earth, and which could not exist if the earth were an exact sphere. Even the amount of the flattening has been deduced from these irregularities with more precision than was attainable by immediate measurements carried on successively in different regions of the globe.

In this we see a striking proof of the progress of science, for not a century has elapsed since the elliptical figure of the earth was still a problem. Not only was the depression of the polar region disputed within the walls of academies, but a theory directly the contrary of this had been proposed and obstinately maintained. In our day all doubts are resolved. Geodesic operations in France, England, Equatorial America, and the British possessions in India; a comparison of the length of the seconds pendulum as observed in different climates; and, as already remarked, the theory of the lunar inequalities—all concur in giving the same value as the measure of terrestrial ellipticity. But this question of the figure of the earth, so prolific of important results, can never be discussed without reference to that great system of operations for which we are mainly indebted to Delambre. Already an associate of most foreign academies, and member of the bureau of longitudes in France, he was elected by the Institute perpetual secretary for the class of mathematical sciences.

We must now advert to the happy union which he contracted with a party every way worthy of him, the mother of a youth named De Pommard, who had accompanied him in all his geodesic expeditions. Under the tuition of this mother, the young man had acquired, in addition to correct principles of literary taste, a familiar acquaintance with the finest productions of foreign literature. Delambre attached himself more and more to this companion of his labors, developed his talents, and enlightened him by his counsels and example. The mother appreciated justly the value of such a friendship, and it is not difficult to imagine how much her heart was touched by the advantages which resulted from it. Having become a widow, Madame de Pommard espoused the friend and protector of her son—the man whose talents and character were honored by the whole of scientific Europe. She honored them herself, for no one could better appreciate high endowments of the heart and intellect. The motives which brought about this union rendered it fortunate; but the family thus tenderly united was not destined to a durable happiness. It was smitten by the unexpected and deplorable loss of the son, who had been the object of so many vows, anxieties, and hopes. His mother found at least as much consolation as the calamity admitted of in the tender affection of Delambre. Thus eighteen

years passed away in the bosom of friendship, confidence, and peace; in congenial occupations, and the exercise of mutual kindness. In the mean time Delambre had succeeded Lalande in the chair of astronomy at the College of France, and was appointed one of the principal *titularies* of the university. For twenty years that he exercised in one of the classes of the Institute and in the Royal Academy of Sciences the function bestowed on him by the suffrages of his colleagues, he cannot be said to have ever once swerved from the line of strict impartiality and equity; and though fidelity to such duties be but the acquittal of an obligation, and no proper subject for formal eulogy, yet it can never be useless to cite as an example that earnestness of purpose which always animated him, or that considerate indulgence which was so much a part of his nature that no personal motive or even injustice could affect it. In his annual reports, in the historic eulogies which he gave to the public, and in his delineations of the progress of science, we everywhere trace the consummate erudition which distinguished him, and recognize a talent for writing formed upon the noblest models. Above all, we would signalize that peculiar temperament which made it for him an agreeable and easy office to exhibit the productions of others in the most favorable light, while at the same time he did not permit himself the slightest deviation from the truth of history.

His literary and scientific labors were so numerous and extensive that we can scarcely be expected to recount them, or even distinctly to explain their objects. Suffice it to say that, besides all the works or memoirs which he published separately, or inserted in the academic collections of Paris, Berlin, and Turin, and in that known as "La Connaissance des Temps," [we have from his hand a complete historical series, comprising "The History of Ancient Astronomy," in 2 vols. 4to., 1817; "The History of the Astronomy of the Middle Ages," 1 vol. 4to., 1819; "The History of Modern Astronomy," 2 vols. 4to., 1821; "The History of Astronomy in the Eighteenth Century," and "The History of the Measurement of the Earth," the two latter having been published since his death; of which works it has been justly said "that no other country has produced anything of the same kind of equal extent and value."] The enumeration of such labors would constitute the most unequivocal title to literary merit, as being calculated to display it in all its brilliancy without effort or exaggeration. If this test were applied to the works of Delambre we could not be deceived as to the rank they must occupy in the history of the sciences. Before him astronomical calculations were founded on numerical processes, which were at once indirect and irregular. These he has changed throughout, or ingeniously remodelled. Most of those which astronomers use at the present time belong to him, having been deduced from analytic formulas, which, in their application, have been found alike sure, uniform, and manageable. The new tables which he has given us of the sun, of Jupiter, of Saturn, of Uranus, and of the satellites of Jupiter, at least some of them, may have been considerably improved by recent labors founded on a greater number of exact observations; yet, in the present state of astronomy, and up to this day, the tables of Delambre just mentioned are those employed in the calculations made for the "Connaissance des Temps" and for the nautical and astronomical ephemerides of most nations. In addition, the geodetic operation, for which we are chiefly indebted to him, and of which he bore the greatest share, is the most perfect and extensive which has been executed in any country. It has served as the model of all enterprises of the kind which have been since projected.

That the labors of Delambre should have had this influence on the method of astronomy is the more remarkable, considering the somewhat advanced age at which he came to the cultivation of the science. He was more than thirty-five when he began to practice observations. The history of the sciences, however, is not without such examples. Newton, it is true, was in possession of all his great mathematical discoveries at an age which Leibnitz had not yet attained

when he first devoted himself to such studies, yet Leibnitz, but a few years later, was one of the inventors of the infinitesimal analysis. But it is to be remembered that both Leibnitz and Delambre had been diligent students from their earliest youth; they had acquired, so to say, the literary habit; their minds had been disciplined to long research. In the works of Delambre we possess an almost complete astronomical library; in his Treatise on Astronomy every known method is reviewed and compared; while his histories of the science furnish a complete deduction of its progress through every stage, from the most remote epochs to the year 1822. The latter portions of this work, unpublished during his life, have received the superintending care of M. Mathieu, an associate of the institute and a former pupil and friend of the deceased.

We will not here recall the questions which have been raised respecting the origin of astronomical knowledge among the ancients. Such a discussion would involve an attentive study of all extant monuments, and the solution of many difficult questions in spherical geometry; it implies, also, a critical examination of the most ancient sources of historical information. Those sources are all indicated in Delambre's work, and his learned analysis can never be dispensed with, whatever be the opinion we may form on the general subject. What, more than all, distinguishes his history of ancient astronomy from the works which preceded it having the same object, is the care which the author has taken to give a clear explanation of the methods pursued by each successive astronomer. These he has rendered by means of the signs employed in modern analysis; and it is remarkable that in rendering he has in almost every instance suggested some improvement in the method.

The long and persevering labors which engrossed him to the last, and from which nothing could divert him—which knew, indeed, no interruption but the few hours of sleep he allowed himself, began to tell more and more upon his health during the last years of his life. The malady through which his services were at last lost to science declared itself in July, 1822; and from the first a fatal issue was foreshadowed by long and frequent faintings and a total loss of strength. He seemed, himself, to foresee the event, preserving to the last instant his unalterable evenness of temper and serenity of mind; so that when the scene finally closed on the 19th of August, 1822, it might be truly said that, though he had suffered much, he had given utterance to no complaint.

Were this the place to recall those scenes of regret and sorrow which followed, what could be added to the noble and touching words pronounced at his obsequies? What tribute of honor to his memory could hope to vie with that in which his illustrious colleague, the witness of his labors and his virtues, embodied the regrets and valedictions of this Academy?*

A sketch has been thus given of the most remarkable incidents in the life of Delambre. If happiness consist in ennobling occupations for the mind; in the exercise of benevolent affections, and the mastery and possession of one's self, what destiny could be happier than his? He enjoyed no exemption from the passing annoyances of life, but its most lasting and desirable blessings were secured to him as the result of assiduous study, disinterested friendship, and inflexible integrity. The consistency of his character was maintained in every situation, as well by the moderation of his desires when deprived of the advantages of fortune, as by the use which he made of those advantages when he possessed them. From earliest youth he had imbibed at its very source a familiarity with all that antiquity has left us of the true and the sublime; and his maturer life was passed in a contemplation of the phenomena of the universe, and in an intimate association with the most celebrated of his cotemporaries. No feelings of hate, no bitter regrets, no ambitious desires troubled his spirit;

* The funeral discourse at the grave of Delambre was pronounced by Cuvier.

offence he offered to none, and envy itself respected his repose. How few are the distinguished men to whom such a lot was granted; and how easily could this reflection be made more striking by a recital of some of the exalted names which preceded him. How melancholy the contrast which is presented in the exile of Tycho, the indigence of Kepler, the illustrious misfortunes of Galileo. Delambre has left works whose extent and nature place him in the first rank of the promoters of science. Both poetry and friendship have been exerted in his praise; and if anything is wanting to his memory, it is a successor who might more worthily have executed the present attempt to do justice to his character and genius. But history, with its eloquent and faithful voice, will redress even this disadvantage in perpetuating to after ages the recollection of so many useful and noble labors.

ESSAY ON THE VELOCITY OF LIGHT.

BY M. DELAUNAY, OF THE INSTITUTE OF FRANCE.

TRANSLATED FOR THE SMITHSONIAN INSTITUTION BY ALFRED M. MAYER, PROFESSOR OF PHYSICS, PENNSYLVANIA COLLEGE, GETTYSBURG.

THE precise determination of the velocity with which light traverses space, first accomplished by the aid of astronomical observations, and then reduced to the proportions of a simple experiment of physics, made in a laboratory of small dimensions, constitutes certainly one of the wonders of modern science. We propose to exhibit in this essay the different phases through which the problem has passed, and the various methods which have been successively employed to arrive at its solution.

We will first endeavor to convey a clear idea of what is understood by the *velocity of light.*

In order to account for the various phenomena of optics, physicists have imagined two systems* in reference to the ultimate nature of light. According to one, every luminous body throws out continually and in all directions into space corpuscles of an extreme tenuity, which in penetrating our eye there produce the sensation called light; this is the *system of emission.* According to the other, light is the vibratory motion of an excessively rare fluid existing in all space, and known under the name of *ether;* a luminous body only produces and keeps up around itself this vibratory movement which is progressively propagated to an indefinite distance; this constitutes the *system of undulation.* Can we certainly say that either system is a true expression of what exists? We cannot. For a long time one system as well as the other accounted for all known optical phenomena; now we know that certain phenomena that we have succeeded in producing cannot be explained by the system of emission, whilst the system of undulation logically embraces them; the latter, therefore, of the two can alone be the true expression of the facts. But it may happen that at some future day we will discover new phenomena which will be no more explicable by the system of undulation than those mentioned can be by the system of emission. But however that may be, it is necessary, in order to give clearness and precision to our reasoning, to represent the phenomena as taking place according to one or the other system, provided we do not absolutely give our mental consent to such a system as being a true expression of existing facts.

In the system of emission the luminous corpuscles thrown out in all directions are in reality projectiles which move uniformly and in straight lines, provided they are not submitted to causes of change of motion, such as those which result from the meeting of ponderable or gross matter. The velocity with which these projectiles move in space is what we call the velocity of light.

* The word *system* is used by the author in this essay to designate a body of doctrine, and, therefore, may mean either *theory* or *hypothesis.* See Prof. J. Henry's Syllabus of Lectures on Physics for the true meaning of these words, which, unfortunately, are so generally misunderstood and thoughtlessly used. An hypothesis may be defined as an *assumed law,* whereas a theory is "the exact expression of the law of a class of facts," and may be termed a *vindicated hypothesis.—Translator.*

According to the system of undulation light is produced by a succession of vibratory motions of the ether which emanate from the luminous body and are propagated indefinitely around it into space. We can arrive at a clear idea of such propagation by observing the circular waves which dilate from a centre of agitation on a smooth liquid surface, such, for example, as are produced when we cast a stone into a calm lake. The luminous waves are spherical, and have for common centre the source of the light; they expand as they move forward, like the circular waves formed on the surface of the water. If we imagine a straight line drawn from the centre of vibration and extended indefinitely into space, each luminous wave will reach the different points of that line along which it uniformly progresses; the velocity of the light is the length the wave runs over on that line in the unit of time. But we can also have a clear idea of the velocity of light without adopting any hypothesis in reference to the manner in which it is produced. If a source of light, a candle for example, is lighted or extinguished suddenly, this instantaneous phenomenon will not be perceived at the same instant at all points of the space from which we can observe it. If we are quite near to the light we will perceive it immediately; if we are far off there will be a certain amount of time between the production of the phenomenon and its perception by the eye, and this time will be so much the longer as the distance between the source of light and the eye is greater. The phenomenon of which we have just spoken takes, therefore, a certain time to *run over* the distance which separates the place of its production from the eye of the observer, and it is natural to admit that it takes equal times to run over equal portions of that distance; that is to say, that it progresses in any direction whatever, like a moving body animated with a uniform velocity. The velocity of light is the velocity with which a phenomenon of light is thus propagated to a distance; or, in other terms, it is the distance at which the eye ought to be placed from the point where the phenomenon is produced, in order that a unit of time may elapse between the instant of its production and the instant of its perception by the eye.

After what has been said, nothing can be easier than to invent the means to determine the velocity of light. It will suffice to proceed as we always do when we desire to measure the velocity of a mass moving uniformly. If we wish to determine, for example, the velocity of a train on a railroad, we place ourselves in one of the cars, and, furnished with a second-watch, we observe how many seconds elapse between the instant of passing one mile-post and the instant of passing the next post following; if we thus find 2 minutes or 120 seconds, we divide the distance of 1 mile or 5,280 feet by 120, and the quotient 44 shows that the train runs over 44 feet in 1 second. In general, we observe the time employed by the body to move over a known distance, and then we divide the distance by the time expressed in seconds, and the quotient is the velocity in one second of time.

In order to apply this method to the measure of the velocity of light, let us imagine two sources of light, A and B, two lamps with reflectors, for example, placed several thousand yards apart. If we suddenly place just before the lamp A a screen, the observer at B will not at that instant see the light at A disappear; he will see it disappear only after the time employed by the light to run over the distance between the two lamps. If at the very instant that the observer at B sees the light A disappear he screens, in his turn, the light B, the observer at A will not see the light B disappear until a short time after it has been screened, on account of the time the light occupies to run over the distance between the two lamps. The interval of time comprised between the instant the lamp A was screened and the instant when the observer stationed at that lamp perceived the disappearance of the lamp B, is, therefore, the time employed by the light to run over twice the distance A B; if we measure this

time by any means whatsoever, we have only to divide double the distance A B by the time found in order to obtain the velocity of the light.

In proceeding as we have just indicated, no matter how great may be the distance of the two lamps A and B placed on the surface of the earth so that either may be seen from the place where the other is stationed, we always find that the time elapsed between the instant the screen is placed before the light A and the instant when the light B disappears to the observer stationed at A is absolutely inappreciable. It appears as if the extinction of the light of each of the two lamps was perceived at the same instant, without any delay, at the station of the other lamp; that is to say, as if the velocity of light was infinitely great. This happens because the velocity of light, although not infinitely great, yet is of a very high value; light going over in one second about three hundred thousand kilometres, (about 190,000 miles.*) or 7½ times the circumference of the earth; it therefore does not employ, in the experiments we have just explained, to go from one lamp to the other but a fraction of a second, absolutely inappreciable by the ordinary means of observation.

Every one has observed that sound requires a certain length of time to come from a distance. If we observe afar off a wood-cutter strike with his axe the trunk of a tree, we see each blow some time before we hear the sound which it occasions. On account of the extremely high velocity of light we can say that we perceive the blow at the very instant it is given by the wood-cutter; the interval of time which elapses between the instant when we see the blow and the instant when we hear it, is therefore the time employed by the sound to come from the tree to the place we occupy. This time, which is very appreciable, is so much the longer as we are at a greater distance from the wood-cutter. If we remove ourselves to such a distance that the interval is exactly one second, then we will have but to measure the distance to the tree in order to have the velocity of the sound. Instead of that, we can proceed as we indicated for light, by replacing the lamps A and B by cannon whose discharge we can instantly bring about. It is in operating thus that we have found that sound runs over 333 metres in one second of time.† There is the greatest analogy between the progressive transmission of light to a distance and the progressive transmission of sound through the atmosphere; only light is propagated with a velocity incomparably greater than sound. The clear conception of the progressive transmission of sound to a distance which we have arrived at from the preceding considerations renders it very easy to conceive the progressive transmission of light; and in order to understand clearly all we are going to say of this last phenomenon we cannot do better than refer to the analogous phenomenon of sound, which is directly accessible to our senses.

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* In reducing the French measures of length to English, we have adopted Captain Kater's value of the metre, the French unit of length, as equal to 39.37079 English inches, or 3.280899 English feet, or 0.6214 of an English mile. The brass scale made by Troughton for the United States Coast Survey has been declared the standard of the United States, and the value of the metre expressed in United States standard inches is given in the Coast Survey Report, for 1853, as equal to 39.36850535 United States standard inches, or 3.28070878 United States feet. Prof. Bache, in 1856, found that the British bronze standard yard, No. 11, is shorter than the American yard by 0.00087 inch.—*Translator.*

† It may be stated, in round numbers, that sound, in dry air and at the freezing temperature, travels at the rate of 1,090 feet, or 363 yards, per second, and every additional degree of atmospheric temperature, on Fahrenheit's scale, adds 1.14 foot to that velocity, so that at 62° Fahrenheit (which is the standard temperature of the British metrical system) it runs over 9,000 feet in 8 seconds, 12¾ British miles in a minute, or 765 miles in an hour, which is about three-fourths of the diurnal velocity of the earth's equator. Hence, in latitude 42½, if a gun be fired at the moment a star passes the meridian of any station, the sound will reach any other station exactly west of it at the precise instant of the same star's arriving on its meridian.—*Sir J. Herschel.*

DISCOVERY OF THE VELOCITY OF LIGHT, BY ROËMER.

During a long period we had no knowledge of the velocity of light. On account of the extreme greatness of that velocity, the phenomena of light produced on the earth being perceived immediately at considerable distances on the surface of the globe, it was usual to consider the passage of light through space as absolutely instantaneous.* The honor of the discovery of the progressive transmission of light and the determination of the value of its velocity is due to Roëmer, a Danish astronomer who was induced by Picard to settle in France in 1672. He made the important discovery by examining and discussing with care the observations on the eclipse of the first satellite of Jupiter.

It is well known that Jupiter, the greatest of the planets which, like the earth, circulates around the sun, is accompanied by four moons or satellites. These satellites move around the planet in the same manner as the planets move around the sun, conformably to the laws of Kepler; their elliptical orbits are in planes slightly inclined to the plane of the orbit of Jupiter. The planets and their satellites have not a light of their own, and we can only see them when they are illuminated by the sun. If any obstacle prevents the light from falling on one of these bodies, it becomes invisible, or, in other words, it is *eclipsed*. This is what happens frequently to each of the satellites of Jupiter. The satellite in circulating around its planet comes to a portion of its orbit which, relatively to the sun, is behind Jupiter; the planet intercepting the rays of the sun, the satellite is eclipsed for a certain time: this is a phenomenon entirely similar to the eclipses of the moon which we from time to time have the opportunity of observing. If we imagine a cone enveloping at the same time the sun and Jupiter, so that the surface of the cone is tangential to the two spheres, the light of the sun cannot reach any point of the space situate within the cone and beyond Jupiter; this space is what we call the *umbra* or umbral cone of the planet. A satellite is eclipsed every time that in circulating around the planet it penetrates this umbral cone; and the eclipse ceases the instant it has traversed it. On account of the large transverse dimensions of the umbral cone of Jupiter due to the size of the planet, and also on account of the slight inclination of the orbits of its satellites to the orbit the planet describes around the sun, these satellites penetrate at each of their revolutions the umbral cone; there is an exception alone for the fourth satellite, (that which is the furthest from the planet,) which passes sometimes above or below the umbral cone without penetrating it, when it passes in the parts of its orbit the most distant from the plane of the orbit of Jupiter.

The eclipses of the first satellite of Jupiter (that which is nearest to the planet) are much more frequent than those of the other satellites, on account of the rapidity with which it describes its orbit. These eclipses are repeated at intervals of about forty-two hours and a half. Also the eccentricity of the orbit of this satellite being insensible, its movement around the planet is circular and uniform: there necessarily results a great regularity in the occurrence of the successive eclipses. These phenomena are easily observed from the earth,

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* The experiment of the lights explained above was made by Galileo, and did not give any result. Subsequently, Descartes thought of a celestial phenomenon which he imagined would be affected by the progressive transmission of light. He saw that if light occupies a certain time to traverse the distance which separates us from the moon, then that satellite, at the moment it is eclipsed by the interposition of the earth between the sun and the moon, should not appear to occupy in the heavens a position diametrically opposite to the sun; it should, on the contrary, appear notably distant from that position. Observation did not show anything similar, and he concluded that light was transmitted instantaneously through space. These negative results only prove that the velocity of light is too great to produce, in these circumstances, effects appreciable by the means of observation employed. They can, at the most, as Huyghens remarks, furnish an inferior limit to the velocity of light, and do not at all prove that this velocity is infinite.

even with telescopes of rather low power, which suffice to render the satellites of Jupiter very distinct when they are not eclipsed. We ought, however, to remark that it is never possible to observe the beginning and the end of the same eclipse of the first satellite, because, as we only see the umbral cone obliquely, a good portion of this cone, at the distance of the first satellite, is hidden from us by the planet itself. If we observe the satellite when it enters the umbral cone, it passes soon behind the planet, which prevents us from seeing it go out of the cone. If at other times we observe it when it comes out of the umbral cone, a short time before, and especially at the moment it enters the cone, it is hidden from us by the planet. When Jupiter is to the west of the sun, the umbral cone, which is always on the side away from the sun, is situate to the west of the planet; we therefore see the first satellite enter the umbral cone, but we do not see it go out, or, to use the language of astronomers, we see the *immersion* of the satellite, but we do not see the *emersion*. When Jupiter is to the east of the sun, it is the contrary that happens: the umbral cone is projected to the east of the planet, and we can observe the emersion whilst the immersion is invisible.

Whilst in Padua, in 1610, Galileo discovered the satellites of Jupiter. The rapidity of the displacement of these little spheres to one side and to the other of the planet, the frequency of their extinction and of their reillumination, naturally excited the curiosity of astronomers, who accumulated observations on them. It was in studying these observations on the immersions and the emersions of the first satellite, in order to arrive at the means of predicting their eclipses, that Roëmer made the capital discovery which we now propose to discuss.

In order to fully comprehend the influence of the progressive transmission of light through space on the observations of the eclipses of the first satellite of Jupiter, and clearly to understand the manner in which Roëmer was enabled to deduce the value of the velocity of light from the numerous observations of this phenomenon which had up to that period been tabulated, let us take an example relative to the progressive transmission of sound in our atmosphere. Suppose that the same sound is repeated regularly at equal intervals during any given time; this sound is produced, for example, by a wood-cutter who strikes with his axe a tree, or by a blacksmith who strikes with a hammer a piece of iron on his anvil. If we are quite near to the spot where the sound is produced we hear and see at the same instant each blow. If we remove to a distance we no longer hear the blows at the same time we see them, and the interval of time which elapses between seeing and hearing the same blow becomes greater as the distance is increased. If, when far off, we come near, then go off again, and then come near, and so on, the interval of time between seeing and hearing the same blow increases as we increase the distance, diminishes as we diminish the distance, increases again with the distance, and so on: it thence results that, notwithstanding the regularity with which the blows of the axe or hammer are given, the successive sounds which we hear have not equal intervals, since the sounds which follow the blows which produce them are at intervals of time which alternately increase and diminish. When we walk towards the wood-cutter or the blacksmith, the sounds which we successively hear are at shorter intervals of time than the blows of the axe or hammer, and the sounds are at greater intervals than the blows when we walk away from the wood-cutter. In walking with a regular step the circumference of a circle in the neighborhood of the place where the wood-cutter or the blacksmith works, we will be sometimes further off and sometimes nearer to the place; and while we observe the blows succeed each other with great regularity, we hear the sounds occasioned by these blows succeed each other sometimes with diminishing intervals, and at other times with increasing intervals. The irregularity in the succession of the sounds is rendered very sensible by the continuous

variation in the time comprised between seeing and hearing each blow; but even when we only hear the blows without seeing them, that irregularity is always detected by the attentive consideration of the successive sounds; it seems as if the sounds experienced a periodic perturbation—that they take place now before, now after the instants at which we ought to hear them so that they would follow each other regularly. An observer placed in the conditions which we have supposed, and ignorant of the conditions of the progressive transmission of sound, and who would thus hear the sounds of the axe or hammer succeed each other now with rapidity, now with slowness, as he walked round his circular path, might well imagine, at first, that the blows themselves had this irregularity; but in thinking of the manner in which the blows are produced, he would have reason to think that such a periodic irregularity in their production was not very probable; and especially as he would remark that the retardation and acceleration of the successive blows coincided always with his going away from or his approach to the place where they originated, he would naturally be led to attribute the irregularity of the succession of sounds to his own change of position, and would thus be led to the discovery of the progressive transmission of sound.

It was in exactly similar conditions that Roëmer found himself when he examined and discussed with care the observations of the eclipses of the first satellite of Jupiter. This satellite, in entering the umbral cone of Jupiter, suddenly loses its light, and in coming out of the umbral cone it instantly regains it. There is, therefore, in the successive immersions and emersions of that satellite, a series of phenomena of light which are reproduced as regularly as the blows of the hammer of which we have spoken above. The astronomer who observes these phenomena is carried by the earth each year over an orbit which differs little from a circle having the sun as a centre. It is true that Jupiter moves at the same time around the sun, so that the place where the eclipse of its first satellite takes place is displaced continually round that central body; but the time of the revolution of the earth around the sun being much shorter than that of Jupiter, the distance of the observer from that planet and from its satellite experiences the same alternative increase and diminution of distance as if Jupiter were stationary. The period of the returns of the observer to the same distance from Jupiter is alone modified; it is about 399 days instead of only one year. If, therefore, light is not instantaneously transmitted to all distances, if it requires an appreciable time to run over a length such as the diameter of the orbit of the earth, we ought to find the effects in the observations of the eclipses of which we have just spoken. If from observations continued through several years we deduce *the mean* interval of time which separates two consecutive eclipses, we ought to find that the time is greater than the interval really observed between two consecutive eclipses while the earth is approaching Jupiter than the interval observed while the earth is going away from Jupiter. If, in using the value of this mean interval of time which elapses between one eclipse and the following and setting out from an eclipse observed when the earth is at its mean distance from Jupiter, we wish to predict the return of future eclipses, it happens that the instants when the eclipses are really observed are sometimes in advance of and sometimes in retard of the predicted epochs, according as the earth is nearer to or further from Jupiter than at the moment of departure from the point of mean distance. The times furnished by the observations for the commencement or the ending of the different eclipses will therefore appear submitted to a periodic perturbation agreeing with the period of variation of the distance of the earth from Jupiter. This is precisely what Roëmer found in discussing the numerous observations which he had in his possession of the eclipses of Jupiter's first satellite; he thus found that light employs 22 minutes to run over the diameter of the orbit of the earth around the sun, and

consequently 11 minutes to run over the distance which separates us from that body. He published his discovery in a memoir presented to the Academy of Sciences of Paris in 1675, to which he had been elected a member a short time after his arrival in France. The following is the account given in the *Histoire de l'Académie*, t. i, p. 213, année 1676:

"It was only from this vast mass of observations that we commenced to see a truth of physics hitherto unknown to all philosophers, and so ignored that the contrary was almost a fixed principle.

"A great number of very exact calculations having been made of the revolutions of the first satellite of Jupiter, and consequently of all its eclipses caused by the umbra of Jupiter, he found that at certain times it came out of the shadow a few minutes too late, and at other times sooner than it should have done, and he could not account for that variation by known principles. In comparing these times with each other, M. Roëmer saw that the satellite came too late from the shadow always when the earth, in its annual movement, was going away from Jupiter, and too soon when it was approaching the planet. From these facts M. Roëmer began to form the ingenious conjecture that light requires an appreciable time to traverse space. That granted, if the satellite appeared to come out of the shadow too late when we were furthest off from it, it did not follow that it really did emerge too late, but its light took longer to come to us, for, so to speak, we had run away from it. On the contrary, when we went to meet it, the time of the satellite in the shadow should appear shorter.

"To test the truth of this idea, he calculated what differences in the emersions of the satellite corresponded to different distances of the earth, and he found that the light was retarded eleven minutes for a length equal to the distance of the earth from the sun. From that datum he announced to the Academy, in the beginning of September, that if his supposition was correct, an emersion of the first satellite, which would take place on the 16th of November following, would happen ten minutes later than it should according to the ordinary calculation.

"The event agreed with the prediction of M. Roëmer. Notwithstanding this success, as the idea was very new, it was not at first generally received. The savans were cautious not to be led astray by the charms of novelty. The satellite has not for the centre of its motion the centre of Jupiter. Moreover, its revolutions are more rapid when it is nearer the sun, and this should produce irregularities in its motion. But these irregularities do not exactly follow those just mentioned. They even imagined another astronomical hypothesis, which would fulfil all the conditions, but it was too unlike anything hitherto known of the heavens. It might satisfy the calculation, but it had not that probability which could satisfy the mind.

"We must, therefore, admit the retardation of light as probable, according to physics, even though not proved by astronomy."

The admirable discovery of Roëmer was made known in an article published in the *Journal des Savants* of Monday, December 7, 1676, under the title of *Demonstration touchant le mouvement de la lumiere trouvé, par M. Roëmer, de l'Académie Royale des Sciences.* This article, accompanied by a figure, gives a clear and sufficiently detailed explanation of the influence of the progressive transmission of light on the observations of the eclipses of Jupiter's satellites.

CONFIRMATION OF THE IDEAS OF ROËMER BY THE DISCOVERY OF THE ABERRATION OF LIGHT, BY BRADLEY.

The ideas of Roëmer on the progressive transmission of light through space, and the explanation that he had thence deduced of the alternate delays and accelerations in the immersions and emersions of the first satellite of Jupiter, although generally admitted by savans, were not, however, accepted without dispute. The point in consideration was the periodic perturbation in the movement of a single celestial body—the satellite. According to the observations this body was sometimes in advance of and sometimes behind the position it ought to occupy according to the preconceived ideas of the nature of its motion; instead of attributing these delays and accelerations to the change of position of the observer and to the progressive transmission of light, could it not be regarded as indicating a real perturbation in the movement of the satellite, due to a cause not yet discovered? The ideas of Roëmer required, therefore, a confirmation which would entirely relieve them of doubt. This confirmation was given in a remarkable manner some time after. It was fur-

nished by the discovery of *aberration*, made fifty years later by the celebrated English astronomer Bradley.

This phenomenon of aberration consists in an apparent displacement which all the stars and planets experience on account of the combination of the velocity of the earth with the velocity of light. In order to explain this we will take another example from among familiar things, and with which we are all acquainted.

Suppose that we are in a railroad car during a rain, without wind, so that the drops falls vertically. When the train stops we see the rain-drops moving, as they really do, each in a vertical line. But when the train runs on the rails, things are completely changed. The drops now seem to fall obliquely, as if a strong wind blew in a direction contrary to that of the train's motion, and obliged the vertical lines, naturally described by the rain-drops, to be inclined in the direction of its action. It is easy to explain this. From the interior of the car we observe what passes outside through the rectangular opening of the window. Let us consider specially a drop of rain which we perceive near the upper angle or corner of the window on the side in the direction of the motion. If the car was stationary we would see the drop descend along the vertical side of the window of which that corner is the upper extremity. But the car moves at the same time as the rain-drop, and at the moment it arrives at the bottom edge of the window sash it is appreciably behind the vertical edge; the quantity it is behind is exactly the distance run over by the car while the rain-drop was descending the whole height of the window-sash. The rain-drop seems to run over a straight line which joins the point where it enters the space of the window-pane and the point where it goes out; and as this straight line has necessarily a certain obliquity, the rain-drop appears to descend obliquely; and all the other drops of rain moving absolutely the same as the one we have just considered, seemed also to move in this oblique line, which is but an appearance caused by our own motion while we look at the falling rain. If, for example, the car progressed exactly the breadth of the window while the rain-drop fell through a distance equal to its height, a drop appearing in the upper corner of the window would seem to run over the diagonal drawn to the opposite lower corner of the pane.

We will remark, in passing, that if we measure the apparent obliquity of the rain, we can thence deduce the ratio of the velocity of the rain-drops to that of the train, since we know the lengths of the paths run over in the same time by the drops and by the car; and if we know one of these velocities, for example that of the train, we can calculate the other, that of the rain. This method, entirely practical, was proposed several years ago as furnishing the means of accurately measuring the velocity of falling rain.

Thus from the simple fact that the observer is in motion, the bodies which move in his neighborhood do not appear to have the motion which they have in reality, the direction of their motion being altered by this circumstance, and that so much the more as the velocity of the observer is greater.

Keeping always our illustration of vertically falling rain, observed from the interior of a railroad car in motion, suppose that the train gradually changes its direction, and from running north to south it changes to run from east to west. On account of the motion of the car the rain appears driven against the train by a wind blowing from west to east. If the railroad is built in the form of a circumference of a circle, so that the train in running over its whole length comes again to the point of departure, the rain which really falls vertically appears from the interior of the car to fall obliquely under the action of a wind blowing successively from all points of the horizon.

circumstances are presented when the rain, instead of falling verti-ith a certain obliquity, caused by a wind blowing regularly. If ws the rain in a direction contrary to that of the car, the motion

of the car causes the rain to appear to fall more obliquely than it really does; if the wind blows the rain in a direction with that of the car, the obliquity with which it really falls appears diminished, and may, according to the circumstances, be annulled or even changed in direction; if the wind makes any angle whatsoever with the railroad, the drops of rain will always appear to fall behind the positions they would have if the car were at rest. In a word, the direction according to which the observer seated in the car sees the rain fall is more than the real direction of the rain modified as it would be if blown by a wind in a direction contrary to the motion of the train. If the car runs over a circular road the influence of its movement on the apparent direction of the fall of the rain-drops changes progressively, so that the rain seems successively to come from different points of the heaven situate all round the point from which it really falls.

If what has just been said is clearly understood, it will not be difficult also to understand the phenomenon of aberration discovered by Bradley, a phenomenon which presents the closest analogy with the apparent change in direction of the rain-drops of which we have just spoken. Replace the rain by the light which comes from the stars, and the car moving on the circular railroad by the earth which annually moves around the sun in an orbit which differs little from a circle. The observer who perceives the light coming from the stars, and who is carried by the earth in its annual movement around the sun, receives that light from positions in space which differ from the directions in which it would come to him if he were at rest. so that to allow the light to traverse the axis of his telescope he is obliged to give it a direction different from what it would have if he were not himself translated with a certain velocity. In the example of the rain-drops observed from the interior of the moving car, if we placed near the window a tube so directed that a drop of rain entering the top would pass through the tube without touching the sides, that tube would have the direction of the lines the rain appears to describe; on the contrary, we would have to give it the true direction of the falling rain if the car did not move. This tube stands in the place of the telescope, which the observer so directs that the light coming from a star traverses its whole length to arrive at his eye placed at the other end behind the eye-piece. If the earth did not move, the telescope ought to point in the real path of the light coming from the star—that is to say, in the straight line drawn from the observer to the star; but the earth being in motion, the telescope has to be directed in the apparent path of the light, a line which indefinitely prolonged into space would not pass through the star, but considerably in advance of it, when we refer its position to the motion of the earth. We therefore see the star in a point where it does not exist, and that apparent deviation, which is but an illusion arising from the motion of the observer in space, changes gradually in direction as the earth moves over the different parts of its annual orbit, so that it returns to exactly the same conditions in the course of a year. Each star, therefore, seems to revolve around its real position, which point we do not see; and the angular distance which separates it from that real position depends not upon the distance which exists between us and it, but solely on the ratio which exists between the velocity of light and the velocity of the earth. If the two velocities were comparable, that is to say, if the ratio of the greater to the smaller was not a very large number of units, which is the case with the velocity of falling rain compared to the velocity of the train, the apparent deviation of which we have spoken would be very considerable, like the deviation we observe in the falling rain when we are in a car, which from a state of rest commences to run rapidly over the rail. If, on the contrary, the velocity of light were incomparably greater than that of the earth, and as infinity compared with the latter, the apparent deviation of the rays of light, due to the velocity of the earth, would be extremely small, and might, on

account of its minuteness, escape our investigations, notwithstanding the precision of the means of observation employed by modern astronomy. But neither of these cases is presented for our consideration; it is true that the velocity of the earth is very small compared to that of light, but not so small that the apparent deviation due to the velocity of displacement of the observer is insensible: this deviation, for its maximum, is about 20 seconds of a degree, a quantity not only appreciable, but also even measurable with great precision by the aid of the instruments which now exist in observatories.

It is in this apparent deviation of each star, a deviation which changes in direction from one period of the year to another, so that the star appears to revolve annually around its real position, that consists the phenomenon of aberration discovered by Bradley. We will content ourselves with having given in what precedes a general idea of the cause of the phenomenon, and we will not endeavor to show how by setting out from the causes indicated we can find the laws it follows during the different times of the year. We will simply state that the total apparent motion which results to each star takes place in an ellipse whose greater axis, as it is for all the stars, is seen under an angle of about forty seconds of a degree; this greater axis is always directed parallel to the ecliptic. As to the smaller axis of the ellipse, it varies from one star to another, and it is so much the smaller as the star is nearer the ecliptic: equal to the greater axis for a star situate at the pole of the ecliptic, reduced to zero for a star situate directly on the ecliptic. The ellipse of aberration, therefore, corresponds with a circle at the pole of the ecliptic, and in going from that pole it becomes more and more flattened as the star to which it corresponds is found nearer to the ecliptic, so as to be reduced to a simple straight line of a length equal to the greater axis for each star situate on this great circle of the sphere.

As it often happens in the science of observation, Bradley discovered aberration without seeking for it. Molyneux had placed at Kew, near London, an instrument constructed with the greatest care by the celebrated Graham, and destined to observe with all possible precision the passage of stars near the zenith. The object that astronomers had in view was to arrive at some precise data of the annual parallax of the stars. We know that the annual parallax of the stars is the displacement with respect to each other which the different stars ought to appear to us to have at different periods of the year, because we successively see them from different points of the orbit of the earth, so that, in going from some and in approaching others, we see them under different aspects as the earth carries us in its motion around the sun. Since the general adoption of the system of Copernicus, astronomers were desirous to prove the existence of that annual parallax which is a necessary consequence, and which would be regarded as an incontestable proof of the truth of this system. With the instrument of Molyneux were made the first observations which led to the discovery of aberration. The following is his account of the discovery, given in a letter to Halley:*

"Mr. Molyneux's apparatus was completed and fitted for observing about the end of November, 1725, and on the 3d day of December following the bright star in the head of Draco (marked γ by Bayer) was for the first time observed as it passed near the zenith, and its situation carefully taken with the instrument. The like observations were made on the 5th, 11th, and 12th days of the same month, and there appearing no material difference in the place of the star, a further repetition of them at this season seemed needless, it being a part of the year wherein no sensible alteration of parallax in this star could soon be expected. It was chiefly, therefore, curiosity that tempted me (being then at Kew, where the instrument was fixed) to prepare for observing the star on December 17, when, having adjusted the instrument as usual, I perceived that it passed a little more southerly this day than when it was observed before. Not suspecting any other cause of this appearance, we first concluded

*A letter from the Rev. Mr. James Bradley, Savilian Professor of Astronomy at Oxford, and F. R. S., to Dr. Edmond Halley, Astronom. Reg., &c., giving an account of a new discovered motion of the fixed stars.—*Philosophical Transactions R. S., No. 406, December*, 1728.

that it was owing to the uncertainty of the observations, and that either this or the foregoing were not so exact as we had before supposed, for which reason we purposed to repeat the observation again, in order to determine from whence this difference proceeded, and upon doing it, on December 20, I found that the star passed still more southerly than on the former observations. This sensible alteration the more surprised us, in that it was the contrary way from what it would have been had it proceeded from an annual *parallax* of the star; but being now pretty well satisfied that it could not be entirely owing to the want of exactness in the observations, and having no notion of anything else that could cause such an apparent motion as this in the star, we began to think that some change in the materials, &c., of the instrument itself might have occasioned it. Under these apprehensions we remained some time, but being at length fully convinced, by several trials, of the great exactness of the instrument, and finding, by the gradual increase of the star's distance from the pole, that there must be some regular cause that produced it. we took care to examine nicely, at the time of each observation, how much it was, and about the beginning of March, 1726, the star was found to be twenty seconds more southerly than at the time of the first observation. It now seemed to have arrived at its utmost limit southward, because, in several trials made about this time, no sensible difference was observed in its situation. By the middle of April it appeared to be returning back again towards the north, and about the beginning of June it passed at the same distance from the zenith as it had done in December, when it was first observed.

"From the quick alteration of this star's declination about this time, (it increased a second in three days,) it was concluded that it would now proceed northward, as it before had gone southward, of its present situation; and it happened as was conjectured, for the star continued to move northward till September following, when it again became stationary, being then near twenty seconds more northerly than in June, and no less than thirty-nine seconds more northerly than it was in March. From September the star returned towards the south, till it arrived, in December, to the same situation it was in at that time twelve months, allowing for the difference of declination on account of the precession of the equinox.

"This was a sufficient proof that the instrument had not been the cause of this apparent motion of the star, and to find one adequate to an effect seemed a difficulty. A mutation of the earth's axis was one of the first things that offered itself upon this occasion, but it was soon found to be insufficient; for though it might have accounted for the change of declination in γ Draconis, yet it would not at the same time agree with the phenomena in other stars, particularly in a small one almost opposite, in right ascension, to γ Draconis, at about the same distance from the north pole of the equator; for though this star seemed to move the same way, as a mutation of the earth's axis would have made it, yet (it changing its declination but about half as much as γ Draconis in the same time, as appeared upon comparing the observations of both, made upon the same days, at different seasons of the year) this plainly proved that the apparent motion of the stars was not occasioned by a real mutation, since, if that had been the cause, the alteration in both stars would have been nearly equal.

"The great regularity of the observations left no room to doubt but that there was some regular cause that produced this unexpected motion, which did not depend on the uncertainty or variety of the seasons of the year. Upon comparing the observations with each other, it was discovered that in both the before-mentioned stars the apparent difference of declination from the maxima was always nearly proportional to the versed sine* of the sun's distance from the equinoctial points. This was an inducement to think that the cause, whatever it was, had some relation to the sun's situation with respect to these points. But not being able to frame any hypothesis, at that time, sufficient to solve all the phenomena, and being very desirous to search a little further into this matter, I began to think of erecting an instrument for myself at Wansted, that, having it always at hand, I might, with the more ease and certainty, inquire into the laws of this new motion. The consideration, likewise, of being able, by another instrument, to confirm the truth of the observations hitherto made with Mr. Molyneux's was no small inducement to me; but the chief cause of all was the opportunity I should thereby have of trying in what manner other stars were affected by the same cause, whatever it was; for Mr. Molyneux's instrument being originally designed for observing γ Draconis, (in order, as I said before, to try whether it had any sensible parallax,) was so contrived as to be capable of but little alteration in its direction—not above seven or eight minutes of a degree—and there being few stars within half that distance from the zenith of Kew bright enough to be well observed, he could not, with his instrument, thoroughly examine how this cause affected stars differently situated with respect to the equinoctial and solstitial points of the ecliptic.

"These considerations determined me; and by the contrivance and direction of the same ingenious person, Mr. Graham, my instrument was fixed up August 19, 1727. As I had no convenient place where I could make use of so long a telescope as Mr. Molyneux's, I contented myself with one of but little more than half the length of his, (viz: of about 12½ feet,

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*The versed sine of an arc A B of a circle is the distance comprised between the extremity A of the arc and the foot of the perpendicular, let fall from the extremity B on the radius drawn to the point A.

his being 24½, judging from the experience which I already had, that this radius would be long enough to adjust the instrument to a sufficient degree of exactness, and I have had no reason since to change my opinion, for, from all the trials I have yet made, I am very well satisfied that, when it is carefully rectified, its situation may be securely depended upon to half a second. As the place where my instrument was to be hung in some measure determined its radius, so did it also the length of the arch or limb, on which the divisions were made to adjust it, for the arch could not conveniently be extended further than to reach to about six and one-fourth degrees on each side my zenith. This, indeed, was sufficient, since it gave me an opportunity of making choice of several stars, very different both in magnitude and situation, there being more than two hundred inserted in the British catalogue that may be observed with it. I needed not to have extended the limb so far, but that I was willing to take in Capella, the only star of the first magnitude that comes so near my zenith.

"My instrument being fixed, I immediately began to observe such stars as I judged most proper to give me light into the cause of the motion already mentioned. There was variety enough of small ones, and not less than twelve that I could observe through all seasons of the year, they being bright enough to be seen in the day-time, when nearest the sun."

Bradley here enters into some details on the results which he obtained in the observations on different stars. He then adds:

"When the year was completed I began to examine and compare my observations, and having pretty well satisfied myself as to the general laws of the phenomena, I then endeavored to find out the cause of them. I was already convinced that the apparent motion of the stars was not owing to a nutation of the earth's axis. The next thing that offered itself was an alteration in the direction of the plumb-line with which the instrument was constantly rectified; but this, upon trial, proved insufficient. Then I considered what refraction might do; but here, also, nothing satisfactory occurred. At last I conjectured that all the phenomena hitherto mentioned proceeded from the progressive motion of light and the earth's annual motion in its orbit, for I perceived that, if light was propagated in time, the apparent place of a fixed object would not be the same when the eye is at rest as when it is moving in any other direction than that of the line passing through the eye and object, and that when the eye is moving in different directions the apparent place of the object would be different."

Bradley examines then, in detail, the apparent positions which the stars should have from the combination of the velocity of the earth with the velocity of the propagation of light through space, and he finds a perfect agreement between the results thus arrived at and those furnished by observation. This change in position of the stars which he had studied with such care was therefore explained, and he was enabled also, from the magnitude of these changes of position, to determine the ratio of the velocity of light to the velocity of translation of the earth. In making this determination from the data given by his observations on the various stars he had studied, he arrives at very concordant results, whence he concludes that the greater axis of the ellipse of aberration, which is the same for all the stars, has an amplitude of forty and a half seconds. It follows that the velocity of light is 10,188 times greater than the velocity of the earth, (the mean velocity being understood,) so that the light ought to take eight minutes and thirteen seconds to come from the sun to us. Bradley, referring here to the discovery of Roëmer, says:

"It is well known that Mr. Roëmer, who first attempted to account for an apparent inequality in the times of the eclipses of Jupiter's satellites by the hypothesis of the progressive motion of light, supposed that it spent about eleven minutes of time in its passage from the sun to us, but it hath since been concluded by others, from the like eclipses, that it is propagated as far in about seven minutes. The velocity of light, therefore, deduced from the foregoing hypothesis, is, as it were, a mean betwixt what had at different times been determined from the eclipses of Jupiter's satellites."

After the discovery of Bradley, the progressive transmission of light became an incontestable fact. Here we do not consider merely the perturbation affecting the position of a single star as in the phenomenon discovered by Roëmer, a perturbation that we might, in truth, regard as really existing in the motion of the satellite, although we might not be able to give the ultimate cause. But in the new phenomenon discovered by Bradley we observe all the stars subjected to a perturbation of the same nature, all following the same laws, and not varying from each other, but by reason of their different positions relatively to the ecliptic. This general perturbation, evidently owing to a single general

cause, irrespective of each individual star, is explained most satisfactorily by the hypothesis of the progressive transmission of light, and the result is in perfect accord with the deductions of Roëmer, from the observations of the eclipses of Jupiter's satellites. Such an agreement of circumstances does not allow of the least doubt as to the truth of Roëmer's conception.

We will remark, in passing, that aberration, which is a result due to the combination of the velocity of light with the velocity of the earth, in its annual movement around the sun, furnished at the same time a proof in favor of the system of Copernicus—a proof which had, up to that period, been vainly sought in the annual parallax of the stars.

This phenomenon, discovered, studied, and so well explained by Bradley, had not remained entirely unknown before his time. In fact, in the work of Picard, entitled *Voyage d' Uranibourg*, speaking of the different values which Tycho had found, for the height of the pole at his observatory of Uranibourg, we read the following passage:

"Besides that, there is an obstacle in the way of the use of the polar star, (for determining the height of the pole,) which, from one season to another, experiences certain variations which Tycho had not remarked, and which I observed nearly ten years ago, which consists in this—that, although the polar star approaches continually the pole* by about twenty seconds, it nevertheless happens that towards the month of April the inferior meridian height of that star becomes less, by several seconds, than it appeared at the preceding winter solstice, instead of which it should have been greater by five seconds, and, consequently, in the months of August and September its superior meridian height is found to be nearly that which it was observed to have in winter, and sometimes even greater, although it ought to have diminished from ten to fifteen seconds; but, *finally, towards the end of the year all is compensated*, so that the polar star appears nearer to the pole by about twenty seconds than it was before."

There is no denying that we have here the annual change of position due to aberration. Picard adds, a little further on:

"To tell the truth, I have not as yet been able to imagine any explanation that satisfies me."

Delambre, who cites these passages in his *Histoire de l'Astronomie Moderne*, t. ii, page 616, accompanies them with the following reflection:

"When Roëmer, brought into France by Picard, measured the velocity of light, he little imagined, notwithstanding the identity of the annual period, that his discovery had anything in common with those variations which had for so long troubled Picard, who, no doubt, had often spoken of them."

In the two phenomena, of which we have successively spoken, the progressive transmission of light does not play the same part. If the observations of the eclipses of Jupiter's satellites unveiled to Roëmer a periodic perturbation, agreeing with the successive augmentations and diminutions of the distance of Jupiter from the earth, that is owing to the time the light employs to traverse the earth's orbit; the velocity of the earth has nothing to do with the amplitude of the phenomenon, which depends entirely on the dimensions of its orbit. In the phenomenon of aberration, on the contrary, it is the velocity of the earth which is principally concerned; and it is of little consequence whether its orbit be small or large. If we transport ourselves in thought to the planet Mars, which describes an orbit of larger dimensions than the earth, but which at the same time moves with a less velocity, we will see that the two phenomena of which we speak will vary as to their magnitude in an inverse manner: the eclipses of the first satellite of Jupiter will appear to experience a greater perturbation, and the aberration of the stars will be less. The contrary will happen if we place ourselves on the planet Venus, which has a greater velocity of translation than the earth, and runs over a smaller orbit: the aberration of the

* On account of the procession of the equinoxes.

[illegible] first satellite of Jupiter [illegible] on a [illegible].

[illegible] The [illegible] make use of [illegible] more or less [illegible] satellites. On [illegible] and of emersions [illegible] necessary, in the irregularities which [illegible] what is due to the eccentricity of the orbit of the satellite, and to the perturbations with which it is [illegible] affected, from what is but an appearance arising from the progressive transmission of light. It is for these reasons that the first satellite is selected as the most proper to furnish the data for the determination of the velocity of light; the greater velocity of its motion and the small depth of the penumbra at the point where it crosses it, causes it to lose its light more rapidly than the other satellites; also the almost entire absence of eccentricity in its orbit renders its motion more regular, and thus gives more prominence to the irregularity due to the time that the light takes to traverse the orbit of the earth. But we can never arrive at but a rough approximation of the velocity of light as deduced from the observations of the eclipses of this satellite, as Bradley has shown in the last paragraph cited from his letter; since the results arrived at by this means, for the time employed by the light to come from the sun to the earth, varies from seven to eleven minutes.

The phenomena of aberration furnish far more precise data for the determination of the velocity of light. As all the stars are affected by aberration, we can multiply at will observations which have a great degree of precision. Bradley estimates that the error in his determination of eight minutes and thirteen seconds for the light to travel from the sun to the earth cannot exceed ten seconds.

Employing the most exact means of modern science, Mr. W. Struve, director of the Central Russian Observatory, near St. Petersburg, made numerous observations during three years (from April, 1840, to the end of 1842) to obtain the exact value of aberration. In discussing these observations, he found that the half of the greater axis of aberration, which is the same for all stars, has an amplitude of 20.45 seconds; whence it results that the velocity of light is 10,089 times greater than the mean velocity of the earth in its orbit.

The earth employing 365¼ days to run over its orbit, light takes 10,089 times less, namely 52 minutes and 8 seconds, to go over a line of the same length; and regarding that orbit as the circumference of a circle, we have but to divide 52 minutes 8 seconds by twice the ratio of the circumference to the diameter (3.1416,) to have the time employed in traversing its radius—that is to say, the mean distance of the sun from the earth. We thus find 8 minutes 18 seconds the number which is now adopted by all astronomers.

The observations of the eclipses of Jupiter's first satellite, and those of the phenomena of aberration, lead directly, as we have just seen, although with a different degree of approximation, to the determination of the time light occupies to run over the mean distance of the sun from the earth. To deduce from this the absolute value of the velocity of light referred to our ordinary units of length, the metre (the foot or the mile,) we must know how many metres (miles) are contained in the distance from the sun to the earth. The value of this distance is found by means of the *parallax of the sun*; we designate thus the angle under which, being at the sun's centre, we would see the radius of the

earth. The sun's parallax, calculated from the observations of the last transit of Venus over the disk of the sun in 1769, is fixed at 8.57 seconds; hence the distance of the sun from the earth is equal to 24,109 times the radius of the earth, or to 153,500,000 kilometres (or to 95,384,900 miles.) As this length is run over by the light in 8 minutes 18 seconds or in 498 seconds, we conclude that the velocity of light is 308,000 kilometres (or 191,391 miles) in one second.

However, for some years several circumstances have conspired to make us believe that the determination of 8.57 seconds given as the value of the sun's parallax is too small, and that the parallax ought to be augmented by a quantity not less than the thirtieth of its value, which would elevate it to about 8.9 seconds. From this increase in parallax results a diminution in the earth's distance from the sun, and consequently in the distance gone over in 8 minutes 18 seconds by the light; the velocity of light will therefore be reduced to a little less than 300,000 kilometres (or 186,420 miles) in a second. The next transit of Venus,* which will happen in 1874, cannot fail to set at rest all doubts which may yet remain on this point.

THE EXPERIMENT PROJECTED BY ARAGO TO DETERMINE THE VELOCITY OF LIGHT.†

The velocity of light, discovered and determined by the labors of which we have just spoken, was entered as an established fact among the truths of science. Further researches could at most only lead to a greater precision in its determination, already approximated so closely by Bradley. The magnitude of that velocity, so great that in one second it runs over more than seven and a half times the circumference of the earth, would cause us to consider the dimensions of the earth as far too small to serve as a basis for an experimental determination of its value; the incomparably greater dimension of the earth's annual orbit was not more than sufficient to reveal to Roëmer, from his observations of the eclipses of Jupiter's first satellite, that prodigious velocity, and we would naturally think that the measure of the velocity of light, which, from its nature, really belongs to the province of experimental physics, would never depart from the domain of astronomy. Nevertheless, as we will soon see, this recently did take place in the most brilliant manner.

The first step in this direction was the most difficult to make, and it required all the daring of genius to attempt it. We find it in an experiment projected by Arago, and communicated to the Academy of Sciences of Paris during its meeting on the 3d of December, 1838. In the project it was not as yet proposed to measure the velocity of light, but simply to compare the velocities with which light moves in air, or in a liquid such as water, or bisulphide of carbon; it was proposed to find by experiment which of these two velocities was the greater, which would decide in an irrefutable manner between the two systems imagined by physicists to explain optical phenomena, viz: the system

* The observation of the transit of the planet Venus across the disk of the sun is by far the most precise means that we can employ to determine the value of the parallax of the sun.

† Before proceeding further in the perusal of the essay of M. Delaunay, it is necessary that all who have not given especial attention to the study of recent optical research, and who desire to appreciate the beauty and importance of the remainder of this essay, should understand *why* light should move faster in water than in air according to the emission theory, and slower in water than in air according to the undulatory theory. This is not explained by the author, and without this knowledge it is impossible to appreciate the excellence of these classical experiments of Arago, of Fizeau, and of Foucault.

We would advise the above class of readers to study the points here spoken of in the "Lectures on the Undulating Theory of Light," by Professor Barnard, Smithsonian Report for 1862. In the admirable "Traité de Physique," by Daguin, Paris, 1862, and in Pouillet's "Traité de Physique," will be found detailed accounts of the apparatus mentioned in this essay, illustrated with engravings. The original memoirs in the transactions of the Academy of Sciences of Paris should also be consulted.—*Tr.*

of emission and that of vibration or undulation. We cannot do better than here allow Arago to speak for himself. The following is what he says in the notice printed in the proceedings of the meeting:*

"I propose to show in this communication how it is possible to decide, unequivocally, whether light be composed of little particles *emanating* from radiating bodies, as Newton supposes, and as the greater part of modern geometers admit; or whether it is simply the result of the undulations of a very rare and very elastic medium which physicists have agreed to call *ether*. The system of experiments which I am about to describe will no longer permit, it seems to me, to hesitate between these two rival theories. It will decide *mathematically*, (I use designedly this expression;) it will decide mathematically one of the grandest and most debated questions of natural philosophy.

"Besides, my communication is the fulfilling of a sort of engagement to the Academy I accepted at one of its last secret sittings.

"I discussed the admirable method, by the aid of which Mr. Wheatstone attempted the solution of the problem of the velocity of electricity over metallic conductors. I had hardly terminated the enumeration of the important results obtained by that ingenious physicist, when several of our members, whose names are authority in such matters, stated that my report was far too approbative. 'In supposing it well determined, the inferior limit assigned by Mr. Wheatstone to the velocity of electricity will not have,' said one, 'any marked influence on the progress of the sciences; besides, limits of the same order, and even more extensive, can be deduced indirectly from various electric or magnetic phenomena. As to the method of the revolving mirrors, it does not seem to be susceptible of application, but to the simple questions already studied by the inventor.' I tried to refute this last opinion. I believed myself that the new instrument, suitably modified, would lead to results that Mr. Wheatstone was not aware of. I already foresaw that, even in supposing it enclosed in the narrow limits of a small room, it could serve to measure the comparative velocities of light moving through air and through a liquid. I was not slow in learning, and without having hardly the right to be astonished or to complain that my assertion had been received with incredulity. Nevertheless, I intend to vindicate it to-day in all its parts.

"Principle of the method: Let a ray of light fall upon a plane polished mirror; it will be reflected, as every one knows, in forming with the surface of the mirror an angle of reflection exactly equal to the angle of incidence.

"Let us now suppose that the mirror turns through an arc a around the point of its surface from which the reflection takes place. If this motion, for example, increases by the quantity a, *the original angle of incidence*, it will diminish as much *the original angle of reflection*. The latter will, therefore, after the displacement of the mirror, be smaller than the first by the quantity $2a$; thus it must be increased $2a$ to render it equal to *the new angle of incidence*; hence that angle increased $2a$ will give the direction of the reflected ray in the second position of the mirror; and thus the incident ray remaining the same, an angular motion a of the mirror occasions a *double* angular motion in the reflected ray.

"This mode of reasoning applies as well to the case where the motion of the mirror, acting in a contrary direction, would *diminish* the first angle of incidence. The principle is, therefore, general; and it is also that of all reflecting nautical instruments.

"The reflection from the plane mirrors can serve to project the luminous rays in all parts of space, without, however, altering the relative positions; two rays parallel before reflection will be parallel after their reflection; those at first inclined to each other 1 minute, 10 minutes, or 20 minutes, &c., will form precisely the same angle after the reflection has deviated them.

"Instead of a single ray, let us consider two horizontal rays setting out from *two neighboring* points situate in the same vertical. Admit that they strike on two points of the median line (also vertical) of a plane vertical mirror. Suppose that this mirror revolves on itself uniformly and in a continuous manner around a vertical axis whose prolongation coincides with the median line just mentioned, the direction in which the two horizontal lines will be reflected will depend evidently upon the moment they may reach the mirror, since we have supposed that it turns. If the *two rays* have *set out simultaneously from the two contiguous radiating points*, they will also reach simultaneously the mirror. Their reflection will take place at the *same instant;* consequently in the same position of the turning surface: consequently as if that surface was stationary with respect to them. Therefore their primitive parallelism will not be changed.

"In order that the rays which primitively were parallel may diverge after their reflection, it is necessary that one of them should arrive at the mirror later than the other. It is necessary that in its course from the radiating point to the reflecting and turning surface, the velocity of the ray should be accelerated, or what will be precisely the same thing, it is necessary (the velocity of the first ray remaining constant) that that of the second should experience a diminution. It is necessary, finally, that the two rays should be reflected one after the other; and, consequently, from two distinct positions of the mirror, forming with each other a sensible angle.

* Comptes Rendus des Séances de l'Académie des Sciences. t. vii, p. 954.

"According to the theory of emission, light moves in water notably faster than in air. According to the wave theory, it is precisely the opposite which takes place: the light moves faster in air than in water. Suppose that one of the rays (the upper ray, for example) has to traverse a tube filled with water before it strikes the mirror. If the theory of emission be true, the upper ray will be accelerated in its progress; it will reach the mirror first; it will be reflected before the lower ray; it will make with it a certain angle, and the direction of the deviation will be such that the lower ray will appear in advance of the other, that it will appear to have been deviated more by the turning mirror.

"Circumstances remaining the same, let us admit for a moment the truth of the wave system. The tube of water will retard the progress of the upper ray; the ray will arrive at the reflecting mirror after the lower ray; it will be reflected not the first, as in the former case, but the second in order, and from a position of the polished reflecting face in advance of the position it had when it reflected the upper ray a moment before; these two rays will make with each other the same angle as in the other hypothesis, except (and we should well remark it) the deviation will take place precisely in an opposite direction; the upper ray will now be in advance, always indicating thus the direction in which the mirror revolves.

"To recapitulate: two radiating points, placed near each other on the same vertical line, flash instantaneously before a revolving mirror. The rays from the upper point cannot reach the mirror until after traversing a tube filled with water: the rays from the second point arrive at the mirror without meeting in their course any other medium than the air. To be more definite, we will suppose that the mirror, seen from the position the observer occupies, turns from the right to the left. Well, if the theory of emission be true; if light be material, the upper point will *appear to the left* of the lower point. *It will appear to its right*, on the contrary, if light results from the vibrations of an ethereal medium.

"Instead of two isolated radiating points, suppose that we instantaneously present to the mirror a vertical luminous line. The image of the upper part of this line will be formed by rays which have traversed the water; the image of the lower part will result from the rays which have throughout their whole course traversed the air. In the revolving mirror the image of the single line *will appear broken;* it will be composed of *two* vertical luminous lines, of two lines, which will not be prolongations of each other.

"The upper rectilinear image, is it behind the one below? Does it appear to its left?

"*Light is a body.*

"Does the contrary take place? The upper image, does it show itself to the right?

"*Light is an undulation.*

"All that precedes is theoretically, or rather speculatively exact. Now, (and here is the delicate point,) it remains to prove that, notwithstanding the prodigious velocity of light, that notwithstanding a velocity of 190,000 miles a second, that notwithstanding the small length that we will be obliged to give to the tube filled with liquid, that notwithstanding the limited velocities of rotation that the mirrors will have, the comparative deviations of the two images, towards the right or towards the left, of which I have demonstrated the existence, will be perceptible in our instruments."

Arago then enters into the most minute details of all the parts of the experiment: the velocity of rotation that can be given to a mirror, the visibility of the image formed by light after having traversed the necessary length of liquid, the possibility of reducing that length of liquid, or the velocity of rotation of the mirror by employing simultaneously several rotating mirrors from which the light would be successively reflected, and also in substituting for water bisulphide of carbon, which acts more powerfully on the velocity of the light, are, on his part, the objects of a thorough examination. He then terminates thus:

"Suppose in the experiment that I propose to execute we make use of electric sparks, or of lights successively screened and unscreened by the use of rotating disks, as their emissions should only last during a few thousandths of a second, it may happen that an observer, looking in the mirror from a given direction, and with a telescope of limited field, will only by chance perceive the light. To this I immediately reply that in renewing very often the apparitions of light—every second, for example—that if, instead of a single mirror, we rotate a vertical prism of eight or of ten facets, that with the concurrence of several observers, placed in different directions, and each with his telescope, we cannot fail to have numerous and clear apparitions of the reflected rays. But these are details on which I shall not dwell to-day. I will reserve for another communication the exposition of the system of experiments in which we will render sensible, and in which we will measure, to a certain degree, the *absolute velocity* of light without having recourse to celestial phenomena."

After the publication of this remarkable notice we could not doubt the success of the experiment it detailed. But, not to interrupt the chronological order, we should speak first of an entirely different experiment, by which the direct

measure of the velocity of light was first accomplished on the surface of the earth.

MEASURE OF THE VELOCITY OF LIGHT, BY M. FIZEAU.

If we refer to the first pages of this essay we will there see indicated a very simple means that was first mentioned as suitable for the determination of the velocity of light, a method which would certainly have long ago furnished the value of that velocity if the transmission of light in space was not effected with such amazing velocity. It is this method, barely modified, that M. Fizeau has put into practice, and which he has rendered completely successful by employing a process as simple as ingenious for measuring the excessively short time that the light takes in running over a distance of several kilometres, or a few miles.

We have supposed that two lamps, A and B, were placed at a distance of several kilometres from each other, and that we shut off suddenly the light of A by means of a screen, then the very instant that the disappearance of the light of the lamp A was perceived at B, we screened B likewise, the interval of time which elapsed between the instant that the lamp A was screened and that when the disappearance of the light of lamp B was perceived at A is evidently the time employed by the light to run over twice the distance which separates the lamps. M. Fizeau replaced the lamp B by a mirror destined to receive and to reflect perpendicularly to its surface the light of the other lamp. In this manner the observer stationed near the lamp A, and looking at the mirror just spoken of, ought to see the image of the lamp A; if this lamp is suddenly screened he should see its image disappear in the mirror, not immediately, but after a certain interval of time—that which the light takes to run over double the distance of the lamp from the mirror.

To measure the interval of time spoken of M. Fizeau made use of a disk toothed at its circumference like a cog-wheel, so that this disk presented on its periphery a succession of equal intervals alternately open and closed. If we rapidly turn such a disk in its plane and around its centre, the closed and open spaces formed by the cogs and the intervening openings will pass successively before the same point in space, and the time employed by each tooth or by each opening to come to the place previously occupied by a tooth or by an opening can easily be reduced to a very small fraction of a second. In placing the apparatus so that the teeth of the turning disk pass in succession before the light of the lamp near the observer, these teeth will act like screens which periodically intercept the light. If the time employed by each opening to come to the place occupied by the next preceding cog or tooth is precisely that required by the light to go from the lamp to the mirror and from the mirror back again to the lamp, the observer, stationed quite near to this lamp and looking through the openings of the turning disk as they pass before his eye, ought to see through each opening the extinction of light caused by the passage of the preceding tooth before the lamp. This eclipse of the lamp, produced during the whole time of the passage of the tooth before its light, is perceived by the observer during the whole time of the transit of the following opening; and as the light of the lamp when it passes through an opening of the disk, after its reflection from the mirror, meets the tooth which is now in the place of this opening, it follows that the observer does not perceive at all the image of the lamp in the mirror during all the time that the disk revolves with the special velocity with which we have supposed it endowed.

After what has just been said it is easily understood what will happen when, the apparatus adjusted, we commence by giving to the disk a gradually increasing velocity of rotation. The disk first turning with an extreme slowness, the observer, who has his eye quite near the side of the lamp and who looks in the direction of the mirror, sees the teeth and the openings of the disk suc-

cessively pass before him; when an opening passes, the light of the lamp passes through, falls on the mirror, and comes back to the eye in passing through the same opening which has hardly been displaced during its passage; soon a tooth presents itself, the light of the lamp is intercepted, and the eye sees nothing more; a new opening follows the tooth, and the eye receives the light reflected from the mirror, and so on continuously; the eye, therefore, receives the light in an interrupted manner, with a series of extinctions caused by the passage of the teeth of the disk. If we increase the velocity of rotation of the disk these successive apparitions of the light are brought nearer and nearer to each other, and as the impression caused by the light on the retina lasts some time, about the tenth of a second, after the light has ceased to penetrate the eye, when the velocity of the disk is so great that each tooth takes less than one-tenth of a second to pass before the eye, the sensation produced by the light which has passed an opening has not yet vanished when the following opening arrives and renews it; therefore there is no interruption in the sensation, and the eye sees the light of the lamp reflected from the mirror without any intermission; only the intensity of that permanent light which it perceives is notably weaker than it would be if the teeth of the disk did not pass before the lamp, since the half of the light which it emits in the direction of the mirror is intercepted by the teeth which pass successively before it. When this continuity in the light is obtained by a sufficient velocity of the disk, we are yet far from the particular condition indicated above as giving the experiment which can serve to determine the velocity of light; the light which passes from the lamp through an opening of the disk can be reflected from the mirror almost without loss, and return to strike the eye after having passed the same opening; the last portion of this light, that which is emitted when the following tooth is on the point of passing before the eye, can alone be intercepted by this tooth when they return from the mirror to the eye. If we still continue gradually to increase the velocity of the disk, these last portions of the total light emitted through an opening which are intercepted in their return by the following tooth, increase little by little in importance relatively to that total quantity of light, and in consequence the intensity of the light received by the eye gradually decreases. Finally, when the velocity of the disk has so increased that the time employed by its circumference to progress a distance equal to the breadth of an opening or of a tooth equals the time the light employs to go from the lamp to the mirror and back again to the lamp, the eye perceives nothing at all. The light sent by the lamp in the direction of the mirror is completely intercepted by the teeth of the disk, as follows: half by the anterior faces of these teeth as they pass before the lamp, and the other half by the posterior faces of these same teeth after the light, having traversed the openings, is reflected back by the mirror. If we increase yet more the velocity of rotation of the disk, each tooth passes too rapidly to intercept the whole of the returning light which passed through the preceding opening and was sent back by the mirror; a portion of that light can therefore pass through the opening which follows that tooth and strike the eye, which begins again to perceive feebly the image of the lamp in the mirror. The velocity of the disk still increasing, the intensity of the image increases also until the disk turns twice as rapidly as it did the moment the eye perceived no light. Now, in fact, all the light which has passed through an opening meets on its return from the mirror the following opening which has taken the position of the first, and it can enter the eye without the disk intercepting the smallest portion. It is easy to see that, by still adding to the velocity of the disk, the image of the lamp, seen by reflection in the mirror, again commences to become faint, again to be totally eclipsed, to reäppear again, and so on as far as it is possible to give increased velocities to the disk. If we observe with care the instant when the first of these successive eclipses is completely produced, and if we then

measure the velocity of rotation of the disk, we can easily calculate the time required by a tooth to take the same position as was just before occupied by the next preceding opening; and, as this time is precisely that employed by the light to run over twice the distance of the mirror from the lamp, it is easy thence to deduce the value of the velocity of light. Such is, in principle, the mode of arrangement of the apparatus of M. Fizeau. We will now give such an account of the details as will convey a clear idea of this apparatus.

In the explanation which precedes, we have supposed that the eye of the observer is placed very near the lamp and looks in the direction in which the lamp sends its light to the mirror, so that the incident ray, setting out from the lamp, and the reflected ray, coming back from the mirror, to strike the eye, follow one and the same path. Such conditions seem very difficult to obtain; but all difficulty disappears by using a transparent glass plate, which, being inclined 45 degrees on the direction of the luminous rays, reflects to a right angle a part of these rays from its surface, whilst the remaining portion traverses it without change of direction. Such a glass plate being placed before the eye, with an inclination of 45 degrees on the line drawn from the eye to the mirror, the lamp can be placed laterally, so as to send its light on the glass perpendicularly to that line; the glass reflects towards the mirror a portion of the light it thus receives from the lamp; that light, after being reflected, returns to the glass plate, part of which passes through it to reach the eye.

Rays of light which emanate from a luminous source diverge to all points around their place of origin; it follows that the further a given surface is from the source of light, the less it must receive. If we therefore do not adopt some particular means in the experiment we are engaged in, the mirror, placed at several kilometres from the lamp, will receive only an insignificant quantity of light; moreover, only a very small part of that light will come back to strike on the eye, so that we would really perceive nothing. In order that the light may not thus be lost in surrounding space by the divergence of its rays, we employ converging lenses placed one near the source of light and the other near the mirror. These lenses are so disposed that the rays radiating from the lamp, after having traversed the first lens, become parallel and form a beam which falls without loss upon the second lens; after having passed through the second lens and converged towards its focus, where is placed the mirror, they are reflected, come back to the lens, which renders them anew parallel for their return to the point of departure; then they pass through the first lens, which causes them again to converge, and finally they are viewed by the eye aided with an ocular. In fact, the two converging lenses of which we speak are objectives of two telescopes placed at the two extremities of the distance over which the light travels, and directed towards each other so that the image formed by the objective of the one is seen at the focus of the other. It is in the interior of the first telescope, between the focus and the occular, that is placed the transparent glass plate inclined 45 degrees, of which we spoke above, to receive and to reflect along the axis of the telescope the light of a lamp placed on the side. As to the mirror which sends back the light to its point of departure, we place it exactly at the focus of the second telescope. M. Fizeau, in his notice read before the Academy of Sciences, (meeting of July 23, 1849,) says:

"This arrangement succeeds very well, even when the telescopes are separated by considerable distances. With telescopes of 6 centimetres ($2\frac{4}{10}$ inches) aperture, the distance can be 8 kilometres (nearly 5 miles) without the light becoming too feeble. We thus see a *luminous point* like a star, and formed by the light, which, setting out from this point, has traversed a distance of 16 kilometres, (nearly 10 miles,) then returned and passed exactly through the same point to reach the eye.

"It is exactly at this point that the teeth of the revolving disk must pass to produce the effects spoken of. The experiment succeeds very well, and we observe that, according to the greater or less velocity of rotation, the luminous point shines brilliantly or is totally eclipsed.

In the circumstances in which the experiment was made, the first eclipse was produced in about *twelve turns and six-tenths of a turn* in one second. With double the velocity the star shines again; with a triple velocity a second eclipse takes place: for a quadruple velocity the point shines again, and so on.

"The first telescope was placed on the terrace of a house at Suresnes, near Paris; the second on the heights of Montmartre, at an approximate distance of 8,633 metres, (28,516 feet, or 5.3645 miles.)

"The disk, carrying 720 teeth, was attached to wheel-work moved by weights, and constructed by M. Froment; a register gave the number of revolutions. The light was obtained from a lamp so disposed as to give a very bright beam."

M. Fizeau, by repeating several times his experiment, found for the velocity of light a value which differed but little from that previously deduced from astronomical phenomena; the mean of twenty-eight observations gave him a velocity of 70,948 leagues of 25 to the degree, or of about 315,000 kilometres (195,741 miles) in one second of time.

The publication of this magnificent experiment was an event in science. The project developed by Arago before the Academy, in 1838, had shown the possibility of rendering appreciable the progressive transmission of light in space, and demonstrated that it would have to run over but a very short distance on the earth's surface to determine thus its velocity. The experiment of M. Fizeau, formed on an entirely different plan, realized for the first time so daring an idea, and furnished at the same time a determination of the velocity of light, whose agreement with that which astronomers had arrived at from sidereal observations was as satisfactory as one could desire for a first attempt of the kind.*

REALIZATION OF THE EXPERIMENT OF ARAGO, ON ONE HAND BY M. FOUCAULT, ON THE OTHER BY MM. FIZEAU AND BRÉGUET.

For nearly eleven years the proposed experiment of Arago had remained as an ingenious suggestion when the labors of M. Fizeau awakened the interest of the learned world in problems of this nature. Some months later (meeting of April 29, 1850) Arago again requested for it the attention of the Academy. After having referred to the object of the notice printed in the *Comte Rendu*, of the meeting of December 3. 1838. he added:

"That communication established that, according to readily admitted hypotheses as to the angular deviations susceptible of being observed in an ordinary telescope, it would not be impossible to determine the comparative velocity of light in bisulphide of carbon and in air, without having recourse to an extreme length of tube, or to a mirror, making more than 1,000 turns in a second. But the mirror which M. Wheatstone used made only 800 turns in the same interval of time.

"It was evident that in this method of observation, and for a given angular deviation, the length of tube containing the liquid ought to be so much the shorter, as the movement of rotation of the mirror is more rapid. This is the reason I propose to add to this deviating motion of rotation, which cannot surpass certain limits, a combination of several revolving mirrors.

"The two rays (one having traversed the liquid, the other the air) strike the first mirror, and form a certain angle; this angle is doubled when the rays fall upon a second mirror turning in the same direction with the same velocity; the angle is tripled if these rays are reflected from a third revolving mirror, and so on. We can thus, by the multiplication of the revolving mirrors, arrive at the same result given by a single mirror turning with a double, triple, &c., velocity of that which it is possible to obtain with the certainty of not destroying the teeth of the wheel, or of overheating the axis.

"My friend, M. Bréguet, jr.. undertook to accomplish this end by means of a mechanism, in which the communication of motion was given by wheel-work. He executed a special arrangement of cog-wheels, the invention of which is due to White. At one of the former industrial exhibitions could be seen the system of these movements.

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* It was for this experiment of Suresnes that the Institute of France awarded to M. Fizeau, at its annual meeting of 1856, the triennial prize of thirty thousand francs, founded by the Emperor for *the work or the discovery which, in the opinion of the five academies of the Institute, has done most honor and service to the country.*

... reflected by the mirror attached to the third piece of wheel-work. ... be identical with those which should be given by a revolving ... per second. From this moment the success of the projected ... doubt. It was only to be regretted that, by the three successive ... different mirrors, the light necessarily experienced a considerable ... therefore, desirable to arrive at the result by a single reflection; ... experiments which I am going to relate seem to lead.

... the causes which prevented us revolving a mirror more than ... M. Breguet proposed to relieve the last axis of the weight of the ... was charged, to turn the axis alone; and he succeeded, not without ... axis 8,000 turns per second. The obstacle which prevented us ... when it carried the mirror, a velocity greater than 1,000 turns per ... It was, one would think, the resistance of the air. I myself ... that cause, and all our thoughts were directed to the means of ... a vacuum. We immediately constructed a metallic receiver destined ... apparatus. This receiver had several apertures, of which one was to ... light after having traversed the two columns of air and of liquid. ... to be the objectives of the telescopes, with which to observe the ... mirror, the necessary communications were established by ... between the apparatus and the driving weight. A special tube put ... in communication with an air-pump.

... and placed upon a stone column in the *meridian room* of the observatory ... to make the observation. * * * * * * * The ... our anticipations, turned hardly any faster in the vacuum than in ... again showed the truth of the proverb, "Le mieux est l'ennemi ... the enemy of good enough.) It was necessary to think of returning ... composed of three pieces of wheel-work and of three separate ... which I had given up only to obtain a greater intensity in the ...

... the necessity of going back to the first method of experiment at the ... sight would not allow me to undertake it. My pretensions, therefore ... to having proposed the problem, and of having given the certain ... These means may, during its accomplishment, experience modifications ... them applicable, with more or less facility, without changing their ...

... which it is useless to give here, Arago adds :

I have delayed a long time the realization of that which I had announced ... in large part to the obligations which M. Bréguet, my collaborator, had ... government for the supply of electric telegraphs, and to the desire that I ... I have already said, with a mirror making 8,000 turns per second.

... I may remain content with the thought that no one will execute, without ... an experiment founded on principles and methods of execution which ... world in their most minute details.

... my publication in the *Compte Rendu*, announced to me that he had ... of my apparatus composed of three successive pieces of wheel- ... mirror. He receives the image reflected by the first rotating mirror ... revolving mirror, but upon a fixed mirror, which sends the ray back to ... this second reflection, the rays fall again upon a fixed mirror, from ... a third time to the turning mirror, &c. It is after the last reflection ... mirror that M. Bessel proposes to measure the angular departure ... more simple than the one I proposed, in so far as it required only ... had the very grave inconvenience of diminishing much more the ... reflections from the mirrors than in the other method. * * * ... without knowledge of the prior communication of M. Bessel, made me ... that of the illustrious observer of Koenigsberg.

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... this state when M. Fizeau determined by his so ingenious experiment ... the atmosphere. That experiment was not indicated in my memoir. ... had the right to make it without exposing himself to the slightest ... consideration of the rights of others.

... on the comparative velocity of light in a liquid and in air, the ... experiment "*I have not yet made any attempt in that direction, and I will not* ... *on your formal invitation.* This loyal reserve could only add to ... the character and the works of M. Fizeau had inspired me, and I ... M. Breguet to lend him one or several of my rotating mirrors.

... M. ... inventive genius is well known to the Academy, came also to ... submit to the test of experiment a modification which he ...

"I can only, in the present condition of my sight, accompany with my good wishes the experimenters who desire to follow my ideas, and to add a new proof in favor of the wave system to that which I have deduced from a phenomenon of interference too well known to physicists to need recalling here."

The communication of Arago, of which we have here given the principal parts, was hardly printed in the *Compte Rendu*, of the meeting of April 29, 1850, when the Academy received, at the following meeting, (May 6,) two important communications on this subject, one of M. Foucault, the other of MM. Fizeau and Bréguet. M. Foucault announced that he had realized with entire success the beautiful experiment projected by Arago;* he made known at the same time the modifications he had given to the mode of arrangement indicated by the illustrious perpetual secretary of the Academy, a modification which had allowed him to arrive at this important result, which gave entire evidence of the truth of the theory of undulation as opposed to that of emission. MM. Fizeau and Bréguet had not progressed so far; they announced to the Academy that their apparatus was adjusted and ready to work, and also showed in what this apparatus differed from the original arrangement of Arago. Finally, a few weeks later, June 17, they returned to announce to the Academy that they likewise on their part had made this remarkable experiment; the result, agreeing with that of M. Foucault, was in favor of the theory of undulation.

Two important modifications had been made by M. Foucault in the arrangement of Arago's apparatus. The first of these changes had for its object to render the realization of the experiment incomparably easier. It is remembered that, in the experiment of Arago, the light had to set out, so to say, instantaneously from two luminous points, or rather from a luminous line shining only during an excessively short time; that one beam of that light, after having travelled in the air, and the other beam in a liquid, were to fall upon a mirror endowed with an excessively rapid movement of rotation; that, finally, after being reflected from this turning mirror, they should arrive at the eye of the observer, furnished for that purpose with a good telescope. The direction of the ray could very easily be determined in advance, since it was that of a line going from the source of light to the mirror; but it is not the same with the direction of the reflected rays which depend essentially upon the position occupied by the mirror at the instant the reflection takes place, and as the motion of the mirror and the reflection of the luminous rays from its surface are independent of each other, it is only by chance that the mirror would be found in such and such a position; the observer, therefore, cannot know in what direction he should place his telescope in order to receive the luminous rays after their reflection. To obviate that difficulty Arago supposed that the observer, being stationed anywhere in the space the reflected rays could reach, and having directed his telescope towards the revolving mirror, would successively repeat a great number of times the instantaneous emission of light towards the rotating mirror, so that the rays from each of these emissions being reflected from the mirror when in different positions, there necessarily would be some which would fall upon the objective of the telescope and thus reach the eye of the observer. It was therefore a fugitive phenomenon which the revolving mirror projected at each instant into the surrounding space, sometimes in one direction, sometimes in another, and which by chance, from time to time, would strike the telescope of the observer. It was to multiply the chances of observation of this fugitive phenomenon that Arago, at the end of his communication of 1838, spoke of substituting for the single mirror a vertical prism of eight or ten facets, and to employ at the same time several observers placed in different positions, and each provided with a

* See, at the end of this essay, the article published by M. Foucault, in the *Journal des Débats*, of May 4, 1850.

telescope. Instead of that, M. Foucault modified the instrument so that the reflected rays which should reach the eye of the observer quitted the revolving mirror in one direction, determined beforehand, so that the observer could station himself so as to receive conveniently all the reflected rays without leaving anything to chance. To accomplish this the light from the luminous object fell upon the revolving mirror, not in an intermittent manner, but continuously, so that it was reflected in all directions around its axis; in one of these directions the reflected light met a fixed mirror on which it fell perpendicularly, and which caused it to return over its path in sending it again to the rotating mirror; there it experienced a new reflection which sent it to the luminous object whence it set out. It was, therefore, near that object that the observer placed his eye to receive the reflected rays, rays which, for convenience, M. Foucault turned to one side by means of a transparent plate of glass inclined 45 degrees, similar to that which M. Fizeau had employed in his experiment of Suresnes. The following are the words of M. Foucault in describing his apparatus:

"A direct ray of light, penetrating a square opening, meets, very near the aperture, a reticule of eleven vertical wires of platinum to the millimetre, (.03937 of an inch;) thence it passes towards an excellent achromatic lens of long focus, placed at a distance from the reticule less than double the principal focal distance. The image of the reticule of greater or less dimensions would be formed on the other side, but, after having traversed the lens, the pencil, before its convergence at the focus, falls upon the surface of the revolving mirror, and, animated with an angular motion double that of the mirror, it forms in space an image of the vertical wires, which is displaced with great rapidity. During a small portion of its revolution this image meets the surface of a concave mirror, whose centre of curvature coincides with the centre of figure and the axis of rotation of the revolving mirror, and, during all the time it passes over its surface, the light which has concurred to form it retraces its path and falls upon the reticule itself, producing there its image, equal to it in size. In order to observe this image without shutting out the original beam, we place obliquely to the beam of light, near the reticule, between it and the object glass, a glass plate, and we observe with a powerful ocular the image thrown to one side. * * * The mirror, in revolving, causes this image to reappear at each revolution, and, if the velocity of the motion of rotation is uniform, it remains immovable in space. For velocities which do not surpass thirty turns per second, its successive apparitions are more or less distinct, but over thirty turns give a persistence to the impressions on the eye, and the image appears absolutely fixed.

"It is easy to demonstrate that the mirror, in revolving with greater or less rapidity, will displace this image in the direction of the motion of rotation. In fact, the light which passes between the wires of the reticule does not return to the wires until it has received from the revolving mirror two reflections, separated by the time it takes to run over double the path from the revolving mirror to the concave mirror. But, if the mirror revolves very fast, this time taken by the light to go and come back, even over the small length of 4 metres, (13.12 feet,) cannot be regarded as inappreciable, and the mirror has had the time to change sensibly its position, which is shown by the change of position in the image formed by the returning beam. Rigorously speaking, this effect takes place as soon as the mirror turns, even slowly; but it cannot be observed until the mirror has acquired a certain velocity, and only when we employ certain precautions in the experiment. All my efforts have tended to render this deviation as apparent as possible.

"The principal obstacle to surmount is that, in so complicated a path, the light cannot converge to the focus in a neat, clear image. The deadening which the pencil experiences, in being reflected twice from a turning mirror of small surface, necessarily destroys the nicety of the image, and produces in its contour an unavoidable mistiness. It is for this reason that we have chosen for source of light the equi-distant linear spaces between the wires of a very fine net. Although the image obtained is never clear, yet it is presented under the form of a system of white and black stripes, similar to colorless diffraction bands, each having a well-defined maximum and a minimum of light. Like the wires of the net, these luminous or obscure spaces are distant from each other one-eleventh of a millimetre, (a millimetre equals .03937 of an inch,) and if, to observe them, we place in the occular a micrometer divided into tenths of a millimetre, the two systems of lines will operate, by their relative displacements, as a vernier, and will permit us to measure in the image, with certainty, a displacement of the one hundredth of a millimetre.

"After the known velocity of light, with an objective of 2 metres (6.56 feet) focus, and in using the double path of 4 metres, (13.12 feet,) we find that we need not give to the mirror an extreme velocity (six or eight hundred revolutions per second) in order to obtain displacements of two and three-tenths of a millimetre. * * * * * *

"Such is the construction of the optical apparatus which has permitted me to show the successive propagation of luminous rays. My first attempts succeeded in the air with a

mirror which made only twenty-five to thirty turns per second, the length of the double path being four metres.

"In order to make the experiment with water, we have only to place between the revolving mirror and the concave mirror a column of this liquid, held between two parallel plates of glass in a conical metallic tube, varnished inside with copal, so that the water would remain clear, to take the necessary precautions that the terminal plates are not strained in their frames, and to obviate the inconvenience of the change of focus by the interposition of a liquid layer of 3 metres (9.84 feet) thickness, having parallel surfaces. In the end we succeeded in easily obtaining, with the feeble and green ray which has traversed the water, an image as distinct as that which is formed without the interposition of the liquid. Therefore it is required but to turn the mirror and to measure with precision its velocity of rotation if we desire to deduce the absolute velocities in air and in water, or to operate simultaneously on these two media if we wish to know only the character and difference of these velocities."

The second modification introduced by M. Foucault, in the arrangement of the apparatus Arago proposed to employ, was in reference to the means of giving to the revolving mirror an extremely rapid movement of rotation and of keeping up this motion a sufficient length of time. In the apparatus of Arago, M. Bréguet produced this movement of rotation by means of wheels with oblique teeth revolving under the action of a weight. M. Foucault substituted the direct action of steam, evolved at a certain pressure from a steam-boiler, on a little turbine fitted to the axis carrying the mirror. The much greater simplicity of this rotating mechanism offered very notable advantages in the execution of the experiments.

In his communication to the Academy, M. Foucault thus announced the results he had already obtained:

"In confining myself to the determinations of the velocity (of the mirror) by the sound, (produced by the action of the steam on the little turbine,) as I have already proved by two successive observations that *the deviation of the image after the passage of the light through the air is less than after its passage through the water*, I have also made another confirmatory experiment, which consists in observing the image formed in part by the light which has traversed the air, and in part by the light which has traversed the water. During low velocities the stripes of the compound image were sensibly the continuations of each other, and, *by the acceleration of the movement of rotation, the image is carried to one side, and the stripes are broken at the boundary line, at the junction of the air image with the water image, the stripes of the latter being in advance* in the direction of the common deviation. Moreover, in taking into account the lengths of air and water traversed, the deviations were seen to be proportional to the indices of refraction. These results demonstrate *a velocity of light less in water than in air*, and fully confirm, according to the views of Arago, the indications of the theory of undulations."

M. Fizeau, on his part, made to the Academy of Sciences, in the same meeting of the 6th of May, the point to which he and M. Bréguet had arrived in their attempts to attain the same result. We reproduce here his communication in full, taken from the *Compte rendu* of the meeting:

"We have undertaken to realize the important experiment of which M. Arago spoke to the Academy at its last meeting, and which he is not able to undertake at this moment on account of the feeble condition of his sight.

"The rotary apparatus of M. Bréguet carries a small mirror of 12 millimetres (.472 of an inch) in diameter, to which can be given a velocity of nearly 2,000 turns per second, and which easily makes 1,200 or 1,500 revolutions.

"The optical arrangement that we employ is founded on the return of the rays on themselves, produced by means of a perpendicular reflection: this is the arrangement which I described in a preceding work.

"The light emanating from a luminous image, formed at the focus of the telescope, traverses the objective, meets the revolving mirror, and is reflected normally from a fixed mirror; then comes back to the revolving mirror, traverses the object glass, and returns finally to the focus.

"The phenomenon produced by the rotation consists in the deviation of the return-image, which is a permanent image resulting from the very rapid succession of instantaneous superposed images; the deviation results from the angular movement made by the revolving mirror, whilst the light runs over double the distance which separates it from the fixed mirror. After having observed the deviations in air for distances which were varied, so as to obtain the greatest intensity of light and the best definition possible, we arranged the experiment so as to observe simultaneously the corresponding deviations of water and of air.

For equal lengths of the two media the ratio of the two deviations should be, after one or the other of the theories of light, either $\frac{4}{3}$ or $\frac{3}{4}$.

"But, instead of taking equal lengths, we can take them so that they will be for the water and for the air in the ratio of 4 to 3. According to the theory of emission, these lengths are equivalent, or are run over in equal times, and the deviations should be equal. According to the theory of undulations, on the contrary, these lengths should be run over in very different times, which are for water and for air in the ratio of 16 to 9, and the deviations should be in the same ratio

"We have, therefore, adopted for water a length of 3 metres, (9 feet 10.11 inches,) and for air a length of 2.25 metres, (7 feet 4.58 inches.)

"The experiment made simultaneously on the two media becomes a very delicate differential experiment, in which it is not necessary to know exactly the velocity of rotation of the mirror. We have only to compare the simultaneous deviations of the two images.

"The apparatus is entirely constructed, but the condition of the atmosphere has not yet allowed us to make the observation, and these experiments require so intense a light that it is not possible to substitute for the sun's rays artificial light. If the sky had been cloudless yesterday or to-day, we would have been able to have presented to-day the result to the Academy. If our experiments are not yet accomplished, it is because we waited until M. Arago should authorize us to engage in researches which belong to him."

Six weeks later, as we have already said, MM. Fizeau and Bréguet announced to the Academy (meeting of 17th June, 1850) the complete success of their experiment. They obtained very neat results; the phenomena observed were altogether in accord with the theory of undulation, and evidently opposed to the theory of emission.

Thus the beautiful experiment of Arago was doubly realized. That which had specially facilitated the success of this experiment, and given a great precision to the observation of the phenomena produced, was the modification of the original arrangement as indicated by Arago, a modification which had for its object to replace the fugitive image observed only by chance in a given direction, by a permanent image produced in a determinate place, where we can observe it with precision and at our ease. M. Foucault rightly attached a great importance to that modification, and in the communication to the Academy, the 6th May, 1850, he claimed its invention in these terms:

"This memoir has also for its object to fix the date of a series of applications of the new method, which consists essentially *in the observation of the fixed image of a moving image.*"

But we may see in the communication of M. Fizeau, which we are about to reproduce, that, in the apparatus which he had devised with M. Bréguet for the execution of the experiment, the same modification is found introduced. We there read in fact:

"The optical arrangement * * * * * * * is founded on *the return of the rays on themselves, produced by means of a perpendicular reflection.*"

And further on:

"The phenomenon produced by the rotation consists in the deviation of the image in its return, *which is a permanent image resulting from the very rapid succession of instantaneous superposed images.*"

M. Fizeau even refers this modification to the arrangement he had adopted in his beautiful experiment of Suresnes, for in speaking of the return of the rays on themselves, produced by means of a perpendicular reflection, he adds: "This is the arrangement which I have described in a preceding work." The communication of the notices of MM. Foucault and Fizeau, at the same academic meeting, constitutes a simultaneous publication which does not allow the slightest priority to be established between them on this subject; but M. Foucault took care to assure that priority in publishing two days in advance, in a political journal, the detailed description of his apparatus.* That which belongs essen-

* See, at the end of the essay, the article published by M. Foucault, in the *Journal des Débats* of May 4, 1850.

tially to M. Foucault is the substitution of the steam turbine in place of the wheel-work employed by M. Bréguet to rapidly turn the mirror, and that modification has also a great importance on account of the facility it gives to vary the velocity of the mirror, to regulate it, and to maintain it at a uniform velocity during as long a time as we wish.

MEASURE OF THE VELOCITY OF LIGHT, BY M. FOUCAULT.

The apparatus which served M. Foucault to determine that light moves faster in air than in water was not devised solely for that comparative experiment; its principal object was to furnish the absolute value of the velocity of light. It was in this point of view that M. Foucault brought it forward in 1850, in indicating in a precise manner the means which he proposed to employ in order to arrive at a certain precision in that measure.

We have seen that in this apparatus the return of the rays upon themselves gives place to the formation of a permanent image which is displaced transversely by a quantity so much the greater as the revolving mirror turns more rapidly. The measure of this displacement of the image can make known the quantity the mirror has turned during the interval of two successive reflections of light from its surface in going and in returning—that is to say, while the light had run over twice the distance of the revolving mirror from the fixed mirror; it is therefore sufficient to know exactly the velocity of rotation of the mirror in order to deduce the time elapsed between these two successive reflections—that is to say, the time employed by the light to make double the journey from the turning to the fixed mirror, and consequently the value of the velocity of light.

The following is the very ingenious method by which M. Foucault was enabled to determine exactly this velocity of rotation, or rather so to adjust the motion that the mirror can be made to turn with a velocity determined beforehand.

A wheel-work mechanism gives a uniform movement of rotation to a disk, toothed like a circular saw. This disk makes exactly one turn in one second. The teeth which arm its contour, and which are accurately cut equidistant, are to the number of 400; so that the time employed by one of these teeth to take the place of the one which has preceded it is exactly the $\frac{1}{400}$th part of a second. We so place the wheel that its border cuts the plane of the field of view of the microscope with which we observe the return image from the mirror. If this field were continuously illuminated, the teeth of the disk would appear to pass before the eye with the velocity of their motion; but it is not thus. The light only comes to the field of the microscope at the instant a reflection takes place from the revolving mirror; this field, and consequently the border of the toothed disk, are only illuminated by successive flashes of light, and those flashes are governed by the rotation of the mirror, which at each revolution sends rays into the interior of the microscopes. If the mirror makes exactly 400 turns per second, then the interval between two successive illuminations of the field of the microscope is exactly equal to the time employed by each tooth to take the place of that which preceded it; so that at the moment of the successive illuminations we always see a tooth of the disk in the same point of the field of view: *the disk appears absolutely immovable.* Suppose now that the mirror makes a little *less* than 400 turns per second; whilst it makes a revolution, each tooth of the disk goes a little further than it ought in order to take exactly the place of the preceding tooth; at the moment of the successive illuminations the teeth which replace each other do not appear any longer exactly at the same point in the field of view of the microscope; they appear little by little in advance in the direction of the motion of the rotating disk, so that the disk appears to have a slow movement of rotation in the direction of its real motion

If, on the contrary, the mirror makes a little *more* than 400 turns per second, the teeth of the disk at the moment they are illuminated appear more and more behind a fixed position, and the disk seems to turn slowly in a direction the reverse of its real motion. When we have so adjusted the velocity of rotation of the mirror that the appearance of the border of the toothed disk which we see in the field of microscope appears immovable, we are certain that the mirror makes exactly one turn while the circumference of the disk progresses one division, and consequently whilst the mirror makes exactly 400 turns per second. It was by adding this improvement or complement to his apparatus of 1850, by substituting compressed air (evolved from the blowing-machine of constant pressure of M. Cavaillé-Coll) for steam as the motive power of the little turbine attached to the axis of the mirror, by increasing the length of the path of the light between the two reflections from the revolving mirror from 4 to 20 metres (from 13 feet 1.48 inches to 65 feet 7.4 inches) by means of successive reflections from intermediate fixed mirrors, by taking every possible precaution to measure the displacement of a few tenths of a millimetre given by the revolving mirror to the position of the image in its return, that M. Foucault, in September 1862, succeeded in determining with a certain precision the velocity of light in air. He thus found a velocity of 298,000 kilometres (185,177 miles) per second, a velocity a little below that of 308,000 kilometres, (191,391 miles,) which result from the value of aberration (20.45 seconds) deduced by M. Struve, from very exact astronomical observations, combined with the value (8.57 seconds) adopted up to this period for the parallax of the sun.

Let us recapitulate the series of labors which has led to so remarkable a result:

Mr. Wheatstone (1834-'36) devised the use of a rapidly revolving mirror, to render appreciable excessively small intervals of time.

Arago showed (1838) how, with the aid of such a revolving mirror, we would be able to determine the difference of velocities with which light traverses air and a liquid.

In the mean while M. Fizeau succeeded (1849) in being the first to render evident the progressive transmission of light and to measure its velocity, by means of an experiment made on the surface of the earth, in the space of a few kilometres, and adopted an altogether different method from that proposed by Arago.

An important modification, introduced (May, 1850) in the arrangement devised by Arago, rendered incomparably easier the observation of the effect due to the difference of velocity of light in air and in a liquid.

Owing to this modification the experiment of Arago was executed nearly at the same time (May and June, 1850,) first by M. Foucault, and then by MM. Fizeau and Breguet.

Finally, M. Foucault, perfecting his apparatus, measured (in 1862) the velocity of light in operating entirely in the interior of a laboratory of moderate dimensions.

An admirable experiment in physics, in which, by the power of intellect and manual skill, we have succeeded not only in rendering sensible, but even measurable, the time employed by light to run over a path of 20 metres, (65 feet 7.4 inches) although this time barely equals the $\frac{1}{15000000}$th of a second! and which, if we repeat it so as to vary its elements, and thus make evident the constant causes of error which affect the result, appears capable of giving a determination of the velocity of light altogether as precise as that which is deduced from astronomical phenomena!

We would be unjust if we did not mention that the success of the experiment of M. Foucault was greatly indebted to M. Froment, the able constructor of the different parts of the apparatus. The clock-work which gave to the

toothed disk a uniform motion of one revolution per second, and the revolving mirror put in motion by the little air-turbine, or *sirène*, ought to take rank among the numerous mechanical wonders which have proceeded from his hands. The most ingenious conceptions remain unrealized if they do not find an artist capable of executing them. Thus history has taken the care to treasure the memory of eminent mechanicians, to whom are due the delicate constructions which have given important progress to science. The name of the illustrious English clock-maker, Graham, remains forever associated with the discovery of aberration and of nutation, made by Bradley, with an instrument constructed by that artist; in the same manner the name of M. Froment will remain inseparable from the beautiful and refined experiment of which we have just spoken, and for which he executed the important and ingenious improvements added by M. Foucault to the original apparatus devised by Arago.

Thus science is in possession of two methods, essentially different from each other, to effect the measure of the velocity of light on the surface of the earth.

The first of these two methods, invented by M. Fizeau, was employed by him in his memorable experiment of Suresnes; but although the number that he thus obtained was very satisfactory from its close agreement with that which is deduced from astronomical observations, still we should only regard this experiment as having established the *possibility* of measuring the velocity of light by employing this method. As soon as the success of this method was made known the Academy of Sciences, on the proposition of a commission of which Arago was a member, determined to construct at its expense an apparatus of large dimensions to turn the toothed disk destined to measure the velocity of light by the method of M. Fizeau, in using every possible precaution to give to that measure all the precision of which it was susceptible. The apparatus ordered of M. Froment was finished, but its adjustment on the terrace of the Observatory was prevented; all the details of the observation to be made between this terrace and a distant station, such as Mont-Valérien or the tour of Montlhéry, had been studied; it was determined to employ telescopes of large aperture, in order to increase the luminous intensity of the observed image, and thus give all the desirable distinctness to the successive eclipses occasioned by the increasing velocity of the disk; it was proposed thus to produce twelve or more successive eclipses, &c., when the death of Arago postponed the execution of this important operation. Let us hope that the postponement will not be indefinite, and that the realization of an experiment which promises such chances of great success will not be delayed.

The second method is that of M. Foucault, who has also obtained by its means a value for the velocity of light which is not far removed from that given by astronomical observation, and agreeing even still closer with it if we adopt the increase which it seems we should give to the value of the parallax of the sun. This method, much more delicate than that of M. Fizeau, appears, on account of its very refinement, more exposed than the latter to the influence of constant causes of error capable of falsifying the result. Let us also hope that M. Foucault will not delay to complete his labors, by studying the influence of these causes of error, so as to deduce a value for the velocity of light the most precise that this method can furnish. The simultaneous use of these two methods of measurement, whose results mutually control each other, is not too much in order to determine with all the exactitude possible the value of an element so important as that of the velocity of the propagation of light through space.

ARTICLE PUBLISHED BY M. FOUCAULT, IN THE "JOURNAL DES DÉBATS," ON THE REALIZATION OF THE EXPERIMENT OF ARAGO.

[Number of Tuesday, April 30, 1850.]

To the Editor—*Sir:* I will not wait for the expiration of the fortnight to give you an account of what most occupied the Academy of Sciences during their meeting of yesterday. All knew that M. Arago was to continue the account of his beautiful researches of polarization and of photometry. The attendance was large, and the Academy recorded at its session a foreign associate and two corresponding members—Mr. David Brewster, Lord Brougham, and M. de la Rive, of Geneva. But what was not expected was, that M. Arago recalled attention to one of the most beautiful projected experiments that the genius of a savant has ever produced, and he declared that, after having conceived it, he had left to the young generation the care and the honor of performing it. This experiment has more than once occupied the attention of the Academy; it proposes to decide, by means of a revolving mirror, whether light moves faster in air than in water, and to seek, in the probable result of this experiment, the confirmation of the theory at present adopted to explain optical phenomena. You may judge, sir, of the emotion with which I heard this generous declaration; I, who for several days had in my hands the experimental solution of this great problem! Nevertheless I thought it proper to postpone to the next meeting the reading of the paper in which I have recorded my results. In the mean time permit me, sir, to announce, in a few words, the results which I have observed.

Light employs more time to run over the same path in water than in air, and the time which it takes to traverse these two different media is shown by the deviation of the ray which is reflected at a given moment from a mirror revolving with a great velocity. All things remaining equal, the deviations were found to be proportional to the indices of refraction of air and of water. It is impossible to entertain the least doubt as to the reality of these results; they have been obtained by two different methods. The two deviations were first observed successively and found unequal for the same velocity of the mirror. They were then observed simultaneously, which rendered the observation still more certain.

Permit me to limit myself to the rather technical expression of these new ... When the columns of the Journal are unoccupied I shall enter into ... developments as will render these propositions more intelligible to your ...

Receive, sir, &c., &c.

LÉON FOUCAULT.

[Number of Saturday, May 4, 1850.]

We published last Tuesday a letter of M. Foucault announcing the success ... experiment originally devised by M. Arago, and which, in giving ... velocities of light in air and in water, accomplished the overthrow ... emission theory in favor of the theory of undulation. The sun having ... during the few days past, they have been able to repeat several times ... experiment in presence of a certain number of French and foreign savants, ... the methods which have insured success are generally known to ... In waiting for the communication which will be given at the ... of the Academy next Monday, we will concisely indicate the fundamental parts of the experiment.

... of sunlight reflected from a heliostat in a fixed direction penetrates ... a dark room; it first passes through a small opening of 2 milli... .0797 of an inch) square, then a reticule extended behind this opening.

and formed of eleven platinum to the millimetre. Passing through this reticule, the beam of light meets an objective of a focus of two metres placed at a distance from the reticule less than the double of its principal focal length, and it tends to form beyond a magnified image of the reticule. But before the formation of this image the converging pencil is reflected from a small mirror which, capable of rapidly revolving around a vertical axis, we will call the revolving mirror. After its reflection, the converging beam will form an image before the mirror at a distance of 4 metres, and when the mirror turns, this image moves in space, describing circles double of the number of the turns of the mirror supposed to reflect from its two faces. In sweeping through space this image meets a concave mirror whose centre of curvature corresponds with the centre of figure of the revolving mirror and with the centre of the axis of rotation; it thence results that during all the time that the image of the reticule falls on the concave mirror the light is thrown back to its point of departure by the revolving mirror and returns to form at the reticule its image of natural size. This image coincides exactly with the reticule, when the revolving mirror being at rest is placed at the proper angle of incidence; but as soon as it moves, the image is deviated and deflected in the direction of the motion. In order conveniently to observe this deviation we place obliquely to the path of the entering beam a glass plate which throws this image to one side. This image appears like colorless diffraction bands, striped with vertical lines, distant from each other the eleventh of a millimetre; they are examined with a powerful ocular, having at its focus a micrometer divided into tenths of a millimetre. The stripes of the image bear the relation to the divisions of the micrometer as a scale to its vernier, so that deviations to the one-hundredth of a millimetre can be read off. Calculation shows that a deviation should be observed for thirty turns of the mirror in a second; and in fact it is seen for that velocity; for greater velocities the deviation is measurable. If we wish to measure the velocity of light in water we place between the revolving mirror and the concave mirror a tube three metres long, filled with perfectly clear water, and its ends closed by plates of glass of parallel surfaces. All things remaining the same, the deviation observed when we interpose the tube of water is always greater than when this tube is not placed between the revolving and the concave mirror. But it is better, to operate simultaneously in the air and in the water, to employ two concave mirrors of the same radius of curvature and both facing the revolving mirror; one destined to receive and send the rays through the water, and the other through the air only. The mirror in revolving causes the two images, corresponding to the two reflections, alternately to appear, but the rapid succession of their apparitions makes them appear superposed; to distinguish them from each other we cover a good part of the height of the concave mirror which reflects the image through the air, which reduces the light of the brighter image; the remainder of the field is occupied by the image which has traversed the water. The vertical stripes of these two images should correspond, and indeed do correspond, for low velocities of the revolving mirror. But as the velocity of rotation increases, the two rays are deflected unequally, the stripes break at the line of junction, and the deviation is greater for the dull and green image which has traversed the water than for the luminous and white image which has progressed only through the air. This last experiment, although difficult to repeat with apparatus improvised in a hurry, has the advantage to appeal directly to the eyes; it has been repeated before several distinguished savants, who, in reference to it, no longer retain the least doubt.

To give to the mirrors rapid and constant velocities M. Foucault uses a small steam-turbine, which was constructed with the greatest care by M. Froment. We cannot at present enter into the details of its construction. It will be noticed hereafter, as well as the applications of this new method of experimenting, when the paper in which it is described has been presented to the Academy of Sciences.

OZONE AND ANTOZONE.

BY CHARLES M. WETHERILL, PH. D., M. D.

SCHOENBEIN, in the year 1840, called attention to the existence of a substance which he named from one of its most striking characteristics, ozone, (*οζω, I smell.)* The peculiar odor in the neighborhood of a good electrical machine when in action, and especially when the electricity issues from a point upon the prime conductor, or is drawn from it as a spark, had been well known. A similar odor had also been perceived accompanying the fall of the thunderbolt.* This phenomenon had been characterized as a phosphoric or a sulphur smell.

Schoenbein called attention to the fact that a similar smell is perceived during the decomposition of water by the voltaic pile, and is observed accompanying the oxygen which appears at the positive pole when the gases are collected separately, and that it is also experienced in many chemical processes, especially in those involving a slow combustion. In explaining the odor of the active electrical machine, it had been assumed that the sensation is due solely to a peculiar action of the electricity upon the organs of smell, and not to the presence of a material substance; but Schoenbein discovered, in the cases alluded to, the existence of a body having the chemical properties of active oxygen—that is, of this gas in its condition of entering most readily into chemical combination, to which he attributed the phenomenon in question, and to which he assigned a characteristic name.

During the twenty-five years which have elapsed since Schoenbein's discovery, this difficult subject has been investigated by many scientists, and especially by Schoenbein himself, by Marignac, De la Rive, Fremy, Erdmann, Berzelius, Williamson, Becquerel, Baumert, and others equally well known in research. While there are few subjects which present a wider field for investigation, or which are more important in their relations to a knowledge of animal life, and to some interesting practical questions in technology, there are few which require a greater patience, or a greater degree of skill in manipulation for their research. It is in consequence of these difficulties that our knowledge of ozone is so limited, notwithstanding the time and labor which have been bestowed upon it. It is the object of the present article to give a brief sketch of what is known respecting this substance, on the authority of the article ozone in Poggendorff's Dictionary, and from the essays of experimentors in various scientific periodicals.

Some time elapsed after its discovery before very definite views were held as to the true nature of ozone. Schoenbein, who for a long time denied that ozone is an allotropic form of oxygen, at first supposed that it was a new body which, in union with oxygen, or perhaps with hydrogen, constituted nitrogen, to which he attributed a compound character. De la Rive imagined that the peculiar smell was due to the action upon the organ, of

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* Homer notices the smell of the thunderbolt. Mohr in Pogg. Ann., xci, 625. Thus, in the Odyssey, book xii, verse 417, and xiv, 307, Jupiter strikes a ship with a thunderbolt, *εν δε θειειον πλητο*, "quite full of sulphurous odor." In the Illiad, viii, 135, Jove hurls a bolt, "with the flame of the burning sulphur," into the ground before Diomede's chariot. In the same poem, xiv, 415, Ajax hurls a rock at Hector, who falls "like a mountain oak struck by lightning, which lies uprooted, and from which the fearful smell (*οδμη*) of smoking sulphur rises."

very finely divided metallic dust of platinum, or of gold, which was separated from the conductors by the electric current, and converted into oxides of the metals; but Schoenbein proved this explanation to be untenable by the experiment of employing hot points for the issuing of the electric charge into the air. Under such circumstances the hypothetical separation of metallic particles and their oxidation should be increased; but, upon the contrary, there was no smell of ozone at all perceptible. Besides this, ozonized air, when shaken in a bottle with water, does not lose its peculiar smell. Hence it is a gas, not absorbable by water, but altered in its character by heat. Marignac and De la Rive subsequently performed experiments which seemed to show that ozone is pure oxygen. As Marignac was unable to obtain ozone by electricity with oxygen unless moisture was present, it was possible that the substance might be peroxide of hydrogen, and this hypothesis became highly probable by the extended and careful research upon this point by Dr. Williamson. This chemist prepared ozone by the electrolysis of sulphate of copper, dried it by passage over chloride of calcium, and passed it through a tube containing copper turnings, heated to redness, which had been first oxidized and then reduced in a current of carbonic oxide. By the experiment, oxide of copper and *water* resulted. Schoenbein now adopted this hypothesis, having been fortified by experiments of his own. At first he regarded ozone as a higher oxide (HO_3) than Thenard's peroxide (HO_2) of hydrogen; but at last retained the latter formula. Schoenbein defended this view pertinaciously, supporting it by experiments and by arguments against that of De la Rive, Marignac, Marchand, Erdmann, Berzelius, Fremy, and Becquerel, who prepared ozone with pure oxygen, (obtained from chlorate of potassa, the gas being washed and perfectly dried,) either by the electrical machine or by an induction current. Marignac, by acting with ozone upon pure powder of metallic silver, obtained nothing but the peroxide of that metal. Hence the conclusion was warranted that ozone is oxygen in an allotropic form. Schoenbein's opinion was based upon his view of the halogen bodies, which, like Berthollet, he believed to be peroxides of unknown radicals, and from the analogy of ozone, in its chemical behavior, to chlorine. He also assumed that no *element* is known which has an action upon the olfactory nerves to call forth the sensation of smell; chlorine, iodine, and bromine being, as he believes, compounds, while phosphorus and arsenic act upon this sense by reason of their passage into the condition of oxides.

Baumert contended that the odorous gas evolved by electrolytic action is essentially different from ozone obtained by the electric spark. Its constitution, according to this chemist, is HO_3; it yields no water to anhydrous phosphoric acid until it has first passed through a tube heated to redness, which he supposed decomposes the HO_3. Baumert, however, in subsequent experiments, acceded to the opinion (derived from their investigations) of De la Rive, Erdmann, Marignac, Marchand, Fremy, and Becquerel, that ozone is an allotropic condition of oxygen.

Schœnbein, in 1858, discovered that if to diluted peroxide of hydrogen a few drops of solution of acetate of lead be added, or that if ozonized oil of turpentine be shaken with the same lead salt, peroxide of lead is formed. The same reaction takes place when ozonized oxygen acts upon basic acetate of lead. When the peroxide of lead thus formed remains in contact with the peroxide of hydrogen, both are reduced; the result being water, protoxide of lead, and oxygen. From this reaction, Schoenbein assumed that the oxygen in the peroxides of hydrogen and of lead exists in an opposite condition of polarity, thus: $HO\overset{+}{O}$ and $PbO\overset{-}{O}$, and that by the union of these molecules of oxygen the ordinary inactive oxygen results. Clausius and De la Rive also imagined a similar molecular condition for oxygen.

Schoenbein designated those bodies containing negatively active oxygen, (or $\bar{O}$), *ozonides;* such are permanganic ($Mn_2O_2\bar{O}_5$) and chromic acids ($Cr_2O_3\bar{O}_3$), and the peroxides of manganese, ($Mn\,O,\bar{O}$), silver, ($Ag\,O,\bar{O}$), and lead, ($Pb\,O,\bar{O}$). He named *antozonides;* peroxides of hydrogen, ($HO\overset{+}{O}$), barium, ($Ba\,O\overset{+}{O}$), and all bodies which contain oxygen in a positively active condition, ($\overset{+}{O}$), and this form of oxygen he called *antozone.*

This chemist succeeded in obtaining antozone ($\overset{+}{O}$) by projecting finely powdered peroxide of barium ($Ba\,O\overset{+}{O}$) into cold monohydrated sulphuric acid. A gas is evolved which both Houzeau and Schoenbein formerly supposed to be ozone, but which has different properties. It blues iodide of potassium starch paper, and smells somewhat like ozone; but when agitated with a little water it loses its odor completely, and forms peroxide of hydrogen, which reaction ozone does not produce. A slip of filter-paper saturated with a mixture of dilute solution of ferricyanide of potassium and a persalt of iron is *speedily* turned blue in antozone gas, but in ozone behaves as in atmospheric air. A very small portion of the gas evolved by this reaction in antozone. This is due to the elevation of temperature, which transforms the antozone into ordinary oxygen.

Another means of distinguishing ozone from antozone consists in a slip of paper imbued with solution of sulphate of protoxide of manganese, which speedily becomes brown in ozone, from the formation of peroxide of manganese. In autozone, not only does this reaction not take place, *but papers browned by ozone are bleached by antozone.*

A distinguishing test may also be found in the behavior of the two gases with permaganic acid, which antozone decolorizes and ozone browns.

Dr. G. Meissner discovered that, if well dried electrified air be passed through water, (which may or may not contain air,) it forms, upon issuing into the atmosphere, a more or less dense cloud or mist. The same phenomenon takes place when electrified air issues into a moist atmosphere.

This cloud is formed by the electrifying of either pure oxygen or of air; but not by pure hydrogen, or nitrogen. It occurs, whether the gas contains ozone or not, but in the latter case to a less degree.

By contact with drying substances, as concentrated sulphuric acid, chloride of calcium, and even concentrated solutions of certain salts, the mist may be caused to disappear; but it forms again by the addition of aqueous vapor. The air left to itself gradually loses this mist-producing property, and if the antozone cloud be confined, the water is, after a while, precipitated upon the sides of the vessel, and can no more be produced by the action of vapor, unless the air be electrified again.

Ozoniferous moist air retains its cloud-compelling property longer than that which does not contain ozone; and, on the other hand, *dry* ozonized air preserves this property still longer. Meissner satisfied himself, by numerous experiments, that this phenomenon of mist is due to antozone, and that electrified air contains both ozone and antozone, the former element being absorbed, and the latter not absorbable by iodide of potassium, or pyrogallic acid. He discovered, also, that antozone prepared by electricity is identical with that obtained by the decomposition of peroxide of barium, as the former, when brought fresh and dry in contact with water, generates a proportion of peroxide of hydrogen. Meissner regards peroxide of hydrogen as a chemical compound of antozone and water; but cloud or mist as a physical aggregate of antozone and vapor of water, in which the chemical affinity of the two bodies is very much weakened. Von Babo supposes the antozone cloud to be in most cases peroxide of

hydrogen, and that it may be formed by the action of ozone also; its disappearance he explains by the gradual precipitation, or decomposition of the peroxide.

Meissner regards the formation of ozone and antozone possible simultaneously by the action of either positive or negative electricity upon ordinary oxygen; but Von Babo believes that only ozone is formed by the current. The latter chemist found that ozonized air experienced no diminution of ozone upon its passage through a solution of permanganic acid, but that this took place immediately if the air came first in contact with substances capable of forming peroxide of hydrogen; also, that ozonized oxygen, free from nitrogen, generated with water neither cloud nor peroxide of hydrogen.

According to Meissner, antozone is formed by all processes of oxidation and combustion in oxygen; and, since the ozone enters chiefly, during combustion, into the oxide thus generated, that the result is principally antozone (or peroxide of hydrogen) in the free state. Antozone is, therefore, according to Meissner, the cause of the cloud in tobacco smoke, the smoke of chimneys, of gunpowder, fogs, and aerial clouds. Meissner found that the fumes of phosphorus in the air are *antozone* clouds, and not nitrate of ammonia, the greater portion of the *ozone* generated by the reaction being absorbed by the phosphorus.

Meissner discovered that air saturated with moisture gives a cloud, upon sudden rarefaction, until the barometric pressure is reduced to eight inches. This corresponds to an elevation of 27,000 feet. By the observations of Kämtz, the average altitude of the lightest and highest clouds, the cirrhi, is 20,000 feet, and their greatest height 24,000 feet.

According to Meissner, water condensed from air or oxygen has the form of *vesicles*, while, when it is separated from other gases, the moisture condenses in the form of *rain* or *solid drops*.

A most curious occurrence of antozone presents itself in the fluor spar of Wolsendorf, in Bavaria. This mineral has a peculiar smell, due to antozone, which Schoenbein found in the proportion of $\frac{1}{5000}$ of the weight of the spar, because five grammes, when rubbed with water, yielded 2.125 milligrammes of peroxide of hydrogen. Since antozone is contained in powder-smoke, it may be surmised that the copious rains which follow great battles are due, in a measure, to this body, the decomposition of the cloud of antozone-water being either a cause or an effect of the electrical excitement in the atmosphere.

Having prepared antozone from an antozonide, Schoenbein sought to obtain ozone from an ozonide, and selected permanganate of potassa for that purpose. Bertazzi had already (Cimento ii, 291) shown that by the action of dilute sulphuric acid, at a low temperature, upon this salt, a gas was generated which had the properties of ozone. Schoenbein, upon a further investigation of the subject, found that peroxide of barium projected into the olive-green solution of permanganate of potassa, and oil of vitriol, of 1.85 specific gravity, evolves a gas which has the smell and chemical properties of ozone. It acts strongly upon the mucous membrane, polarizes platinum negatively with great power, and destroys organic coloring matter and pyrogallic acid at the ordinary temperature. R. Boettger, in calling attention to this experiment, states that he had two years previously described the effect of oil of vitriol and permanganate of potassa in producing a long-continued evolution of ozone. He considers the addition of peroxide of barium superfluous, and, for the development of ozone, adds two parts of dry powdered permanganate of potassa to three of hydrate of sulphuric acid in a bottle. By this reaction a strong ozone smell is at once perceived, and all of the characters of the gas may be established by the proper reagents.

Schoenbein has determined that antozone has a density less than hydrogen, and that it liouefies at a pressure of 150 atmospheres. Ozone and antozone,

exposed to the dark rays of the spectrum, unite, with explosion, and yield ordinary oxygen.

Schoenbein considers that oxygen undergoes chemical polarization in the body when respired, and accounts thus for the rapid changes which take place in the tissues. He has found peroxide of hydrogen in the urine, and has discovered that the blood corpuscules instantly decompose this compound. He has given the following test for the peroxide of hydrogen. To water, supposed to contain the peroxide, he adds one or two drops of a solution of a salt of bismuth, nickel, cobalt, thallium, &c., and then just enough of potassa to precipitate the hydrated oxide of the metal. He then adds a little iodide of potassium and starch, and lastly one or two drops of acetic or dilute sulphuric acid. If the merest trace of peroxide of hydrogen be present, the liquid is instantly colored blue.

The following is Schoenbein's method of determining quantitatively the decomposition of oxygen into ozone and antozone. During the slow oxidation of metals, according to this chemist, the oxygen is chemically polarized; one of its atoms (ozone) unites with the metal or oxidizable matter, while the other atom (antozone) combines with water to form peroxide of hydrogen. Schoenbein agitates an amalgam containing five per cent. of lead, with very dilute sulphuric acid of known strength, in a large bottle partially filled with air or oxygen. After a few moments of agitation, a quantity of sulphate of lead is produced, and peroxide of hydrogen is found in the acidulated water. He ascertains the oxygen which has combined with the lead, by determining the amount of uncombined acid, thus calculating the sulphate of lead formed, and consequently its oxygen. He determines the peroxide of hydrogen in another portion of the water by a standard solution of permanganate of potassa, and finds the quantity of oxygen in this peroxide to be very nearly the same as that contained in the oxide of lead.

Different chemists have determined the diminution of volume experienced by air or oxygen during ozonization.

Andrews and Tait placed pure dry oxygen in a tube and discharged electricity through the gas. With still discharges a diminution took place, which was at first rapid and then slow until a maximum was reached, yielding a diminution equal to $\frac{1}{12}$ of the original volume. When, now, a few sparks were passed through the gas it expanded $\frac{2}{3}$ of the former diminution, but did not attain its original volume. With rapid or spark discharges, oxygen experiences a diminution of volume, although less than by still discharges. Oxygen, contracted by the formation of ozone, when left to itself at the ordinary temperature, expands again gradually. At 100° Centigrade it expands more rapidly, and at 270° it regains its original volume and loses all of the characteristics of ozone. At this temperature, therefore, ozone is destroyed. Andrews and Tait found that, by still discharges, oxygen cannot lose more than $\frac{1}{12}$ of its volume, unless the ozone be removed as fast as it is generated, in which case the diminution may proceed indefinitely. They determined 60 for the density of ozone compared with oxygen, which makes it six times lighter than lithium. They were unable to condense it to a liquid at ordinary pressures by a freezing mixture of solid carbonic acid and ether.

Ozone may be prepared for examination most readily by dropping, in small quantities, dry permanganate of potassa in a bottle containing a little oil of vitriol, or by placing a stick of phosphorus, scraped clean under water, in the bottom of a capacious vessel containing enough lukewarm water to half submerge the phosphorus. In either of these cases a slip of iodide of potassium starch-paper becomes instantly blue when immersed in the air of the vessel, and the different ozone reactions may be readily perceived by employing the appropriate tests. The gas may also be investigated with the above starch-paper in the neighborhood of a point upon the prime conductor of an elec-

trical machine, from which still discharges are issuing. It may be obtained by the inductive current of the Ruhmkorff coil, or by the electrolysis of water, using a powerful Bunsen or Grove battery, and employing gold or platina for the positive electrode, from which proceeds a current of oxygen, laden with ozone. The hydrogen gas, issuing from the negative electrode, possesses no odor of ozone, but this smell is perceived when the two gases are collected together. The antozone, formed here by the polarization of the oxygen, unites with the water to peroxide of hydrogen. If the water thus electrolized contain substances capable of union with oxygen, as hydrochloric, hydriodic acids, and their salts, also sulphurous acid, sulphuretted hydrogen, coal, iron filings, &c., or if the liquid or electrodes be heated, no ozone smell is perceived.

There is no process for obtaining pure and isolated ozone, although a constant current of air laden with this gas may be procured for a certain time by the action of sulphuric acid upon permanganate of potassa, or by passing moist air through a tube containing pure phosphorus, and washing the gas with water. The generation of ozone by the electric discharge is so slow that Baumert, by passing 500,000 sparks in an hour, obtained only enough of this gas to liberate one milligramme of iodine from its potassium salt.

Circumstances influence the production of ozone by the electrical discharge. Under the same conditions, sparks of 25 millimetres in length generate twice as much ozone as sparks of 4 to 5 millimetres long.

Employing hermetically sealed tubes of $\frac{8}{10}$ millimetres diameter and 70 millimetres long, $\frac{2}{3}$ of the oxygen can be converted into ozone; but by continuing the discharges the ozone diminishes, so that in 24 hours it equals only $\frac{1}{8}$ of the original oxygen.

By the electrolysis of water, containing $\frac{1}{10}$ of sulphuric acid, Baumert obtained only one milligramme of ozone in 150 litres of the mixed gases; but when the water contained chromic acid instead of sulphuric, he found the same quantity of ozone in 10 litres of the mixed gases.

Ozone is generated by means of phosphorus only in a moist air, containing oxygen at a medium temperature. When the atmosphere is deprived gradually of its oxygen, by means of ignited oxide of copper, the production of ozone diminishes, and ceases entirely when there is no longer any oxygen present.

Ozone is not formed in pure carbonic acid or hydrogen gases. With 1 volume of oxygen and 4 of carbonic acid, the formation of ozone is easy. In an explosive mixture of hydrogen and oxygen, the generation of ozone is powerful; the phosphorus shines vividly, and can acquire heat sufficient to kindle and explode the mixture of gases. In pure oxygen, at the ordinary pressure of the atmosphere, phosphorus, by the presence of water, does not produce ozone until the temperature is raised from 75.2° to 86° Fahrenheit. At this temperature the phosphorus begins to emit light, and the illumination and ozone generation are powerful at 96.8° Fahrenheit.

Oxygen expanded to four times its volume, by the air-pump, yields ozone, by phosphorus, at the ordinary temperature; but at 32° Fahrenheit there is no production of ozone. In dry air phosphorus generates ozone slowly, as the phosphorus soon becomes coated, which prevents the action of the air. There is a connexion between the shining of phosphorus and its generation of ozone, so that this production exists in proportion to the intensity of the illumination; but we are ignorant of the cause of this connexion.

If we well wash air ozonized by phosphorus and place in it a piece of carbonate of ammonia, so that the gas reacts alkaline, and wash the gas again, it still gives all of the characteristic reactions of ozone.

According to Schoenbein's experiments, 1,000 grammes of phosphorus are capable of converting 1,720 of oxygen into ozone. By another trial he gives 0.43 gramme as the yield of ozone by 1 gramme of phosphorus. By hanging silver leaf in a vitriol carboy filled with ozonized air, he could deozonize the

same in four hours; and, by repeating the process for several weeks, he was able to prepare as much as 20 grammes of the peroxide of silver. Ozone results also from the slow oxidation of pure ether, effected by the means of a platinum spiral heated to redness and placed in the vapor of the ether. According to Schoenbein the combustion of hydrogen, carbide of hydrogen, and of kindred gases, produces ozone.

The odor of ozone is so powerful that air containing only one millionth of it has a decided smell of the gas. Ozonized air raised to the temperature of 482° to 572° Fahrenheit, is deprived instantly of this body.

A very characteristic action of ozone consists in its polarizing effects upon the metals, particularly upon platinum, gold, and, to a less extent, copper. An ozoniferous atmosphere behaves like one containing chlorine or bromine. If a slip of gold or platinum is immersed in an ozone atmosphere, it acquires a *negative* polarity in a few seconds. When such a slip is united with an ordinary one and the pair is plunged in acidulated water, a current is generated which acts powerfully upon the needle of the galvanometer. Antozone, also, polarizes platinum negatively, but less so than ozone.

In the preparation of ozone by phosphorus, an odor characterized as "garlic" is first perceived. Gold or platinum, immersed in such air, is polarized *positively*, and this condition may remain for a considerable time if the temperature be low. Presently, with a temperature of 53.6° to 60.8° Fahrenheit, the gas exchanges its "garlic" for an ozone smell; and, at the same time, the metal, after quickly passing Volta's point of indifference, acquires a *negative* polarity. The least moisture deposited upon the metal, also an elevated temperature, prevent, or, if present, destroy this polarization. It is also lost in the air gradually, and immediately in hydrogen gas, which even communicates a positive polarity.

Ozone is absorbed neither by water, caustic ammonia, nor baryta, sulphuric acid, or dry chloride of calcium.

Schoenbein attributes many of the reactions of oxidation by the "*nascent state*" of the gas to the presence of ozone.

Dr. Phipson has given many of such examples of polarization explanatory of the "*status nascens*" in a prize essay which is contained in the Smithsonian Report for 1862.

A paper saturated with a solution of iodine is quickly bleached in an ozone atmosphere; the product, however, does not react acid, and in contact with oxidizable bodies, such as sulphurous acid, sulphide of hydrogen, phosphorus, hydriodic acid, metals like zinc and tin, or protoxides like those of tin or lead, the paper becomes brown again from iodine.

Solutions of chlorine and bromine act in the same manner; and it is this fact, together with the chemical analogies existing between ozone and the halides, which led Schoenbein to assign to ozone a similar nature.

In the presence of strong bases, such as hydrates of lime or potassa, the nitrogen of the air may be oxidized to nitre by the action of ozone. Three thousand litres of ozonized air, agitated with milk of lime, furnish 5 grammes of saltpetre. Ozone liberates iodine from the iodide of potassium more freely in the sun than in the daylight; but when starch is present, the blue iodide of starch is bleached by the light. By renewed action of ozone it becomes blue again, and the bleaching and bluing may be repeated until the iodide of potassium is transformed completely into iodate of potassa; when ozone is no longer capable of producing the blue tinge. According to Baumert, the first effect of ozone upon this salt is the decomposition to caustic potassa and iodine, which react to form iodate of potassa and iodide of potassium. The ozone acts upon this iodide as before, until at length the halide is completely oxidized to iodate of potassa.

The yellow prussiate of potassa is converted quickly into the red prussiate

by ozone. A crystal of the former salt placed in an ozone atmosphere becomes gradually red, the color proceeding from the outside to the inside, and the solution of ferrocyanide of potassium experiences this change very readily when agitated with ozone.

The metals arsenic, antimony, iron, zinc, tin, lead, bismuth, silver, and mercury, are oxidized instantly by ozone, and deprive thereby any atmosphere containing that gas.

The metals are not oxidized with equal facility. Thus arsenic is oxidized much more readily than antimony, which affords an additional means of distinguishing between these bodies in judicial inquiries. An arsenic spot in a strong ozone atmosphere is converted instantly to arsenic acid, while an antimony mirror requires several days for its disappearance. Dry mercury, or that covered with water, does not absorb ozone; if the metal be merely moistened, it takes up ozone readily, becoming yellowish upon the surface.

Silver, in the state of leaf or sponge, absorbs ozone quickly, as has been stated already. This metal may be employed for separating ozone from ordinary oxygen.

The more readily oxidized metals, as protoxides of manganese, iron, tin, &c., as well as those which, like protoxides of silver and lead, are not so easily changed to a higher state of oxidation, are peroxidized by ozone.

A solution of the sulphate of the protoxide of manganese is browned instantly in ozone from the above cause, and, according to Schoenbein, this reaction is perceptible in a dilute solution of sulphate of zinc, which contains as an impurity only $\frac{1}{1000}$ of manganese. If hydrated oxide of lead be spread upon paper, or if basic acetate of lead be employed, ozone will communicate to the paper first a yellow, then a red, and finally a brown coloration from the peroxidation.

If the lead paper be blackened by means of sulphide of hydrogen, it may be bleached in ozone, which is due to the production of the white sulphate of lead.

Organic bodies are very readily oxidixed in ozone, as shown by the experiments of Schoenbein, Baumert, Gorup von Besanez, &c. Thus wood, straw, cork, starch, humus, vegetable colors, caoutchouc—pure and vulcanized—the fats and fatty acids, alcohol, albumen, blood, &c., have been found to be all acted upon by this agent. Ozone bleaches litmus without first reddening it. Certain fungi become blue in the air, and the cut surface of an apple becomes brown by the action of ozone.

Gum guiacum, which becomes gradually blue in the air, owes this change to ozone, as was first shown by Schoenbein, who employed this substance as a reagent for ozone. To prepare guiacum papers, one part of the gum is dissolved in thirty parts of 90 per cent. alcohol. One gramme of ordinary alcohol is charged with a few drops of this solution, and the paper slips are dipped therein and dried. The papers are blued rapidly in an ozone atmosphere. They lose this color when exposed to the air, but regain it in ozone, and the process may be repeated until the guiacum is fully oxidized. Certain metals and other bodies, especially nitrous acid, chlorine, bromine, peroxides of manganese, and lead, &c., will blue guiacum tincture.

The following is Schoenbein's method of determining ozone quantitatively in a mixture from its bleaching effect upon the solution of indigo: The ordinary solution is diluted with sufficient water to be just opaque blue. One hundred grammes of this solution receive an equal weight of hydrochloric acid, and are then boiled. The heated liquid is treated with a solution containing $\frac{1}{100}$ of chlorate of potassa until the blue color is exchanged for a brownish yellow. If, now, for the destruction of the 100 grammes of indigo solution, 100 milligrammes of chlorate of potassa (which contain 39 milligrammes

of oxygen) were employed, then 1 milligramme of oxygen would destroy $\frac{100}{39}=2.564$ grammes of indigo solution. Now, to this last quantity of the original indigo solution, containing hydrochloric acid, enough water must be added to dilute it to 10 grammes,* in order to obtain a normal solution of which 10 grammes will represent 1 milligramme of oxygen or ozone. One part of the above normal solution may be further diluted with 9 parts of water to yield $\frac{1}{10}$ normal test, in which 10 grammes will indicate $\frac{1}{10}$ milligrammes of ozone.

The analysis is performed as follows, for example, upon a bottle of air ozonized by phosphorus: A little of the normal indigo solution is agitated with the air in the bottle for a few minutes, and the air is then tested for ozone by iodide of potassium starch, and the process is repeated until no ozone is indicated even by a slightly elevated temperature. Then, for every 10 grammes of the normal indigo solution employed, 1 milligramme of ozone is present, and for every 10 grammes of the $\frac{1}{10}$ normal solution, $\frac{1}{10}$ of a milligramme of ozone has been found.

Schoenbein discovered the remarkable bleaching properties upon indigo and other plant colors of certain bodies which appear to absorb ozone, without combining with it, and thus are in a position to yield the ozone again to other substances. These bodies have been called "*ozone carriers.*" They are oil of turpentine, linseed oil, oil of lemons, tartaric and citric acids, ether, platinum sponge and black, powder of silver, gold, and platinum, also metallic mercury. These substances, shaken with solutions of indigo, litmus, cochenille, &c., discharge the color; with tincture of guiacum they yield a blue tinge, and give the same color with iodide of potassium starch. One of the most remarkable of the ozone carriers is the blood corpuscule. Ozonized oil of turpentine, shaken with indigo solution, bleaches it after a while, but if blood corpuscules are added, the decoloration is instantaneous. Platinum black is also very active in its bleaching effects.

One of the best known ozone carriers is the oil of turpentine. If a bottle be ¼ filled with this body, exposed to the sun, and frequently shaken with air, removing the stopper from time to time to renew the air, the oil is speedily laden with ozone. After a time the oxidizing or bleaching effect of this oil is lost, probably from its ozone entering into combination with it. Its bleaching effect may be ascertained quantitatively by means of a solution of indigo. Schoenbein prepared an oil of which 1 gramme possessed the same bleaching power as 2 grammes of the best bleaching powder, (hypochlorite of lime.) The ozonized oil bleaches most rapidly when warm, but its absolute decolorizing power is then less. All turpentine kept with access of air becomes ozonized, as may be seen from the bleached and corroded corks employed in closing the vessels containing it. Upon the animal organism ozonized oil of turpentine acts more energetically than oil not ozonized. According to Seitz, 5 drops of ozonized oil throws into convulsions and kills a frog and destroys other small animals, while 20 drops of the pure oil produce no such effect. Not long since a statement prevailed that the disease called painter's colic is due, not to the lead absorbed, but to the oil of turpentine. If this be so, the ozonization of the turpentine may be the active agent for mischief, although the lead may also be hurtful.

The existence of ozone in the atmosphere has been known since the discovery of the body; but the conditions which govern its presence, as well as the actual quantity of the gas, are yet undetermined. This arises from the small amount of ozone in the air, from the readiness with which this active oxidant is destroyed by the presence of bodies upon which it may act, and also for the want of a re-

* Hence, in this case, 10—2.564=7.436 water will be needed.

liable quantitative test. The largest proportion with which the air has been artificially loaded with this substance amounts to $\frac{1}{1300}$ of its volume.

Ozone may be determined quantitatively by ascertaining the amount of iodine liberated from iodide of potassium; by determining the quantity of peroxide of silver which it produces; or by finding the proportion of indigo solution which it decolorizes. But this kind of test has never been applied to a great extent in determining the atmospheric ozone in long series of observations, owing to the difficulty and labor of applying it.

A large volume of air brought slowly to act upon a small proportion of the reagent is necessary for this purpose. In aspirating twenty-four litres of external air during a period of two and a half hours, concentrating its action upon a circle $\frac{1}{32}$ of an inch in diameter on a delicate iodide of potassium starch paper, I was unable to detect the slightest ozone reaction, although a slip of similar paper exposed all day to the free atmosphere was colored sensibly.

The ordinary mode of observing ozone in the atmosphere consists in ascertaining the amount of color produced upon paper containing iodide of potassium and starch, using precautions against the bleaching effect of the light upon the iodide of starch developed by the ozone. A scale of shades of color is employed for determining, by comparison, the proportion of the gas in question present in the atmosphere. Boehm found that this test, prepared from the same recipe by different persons, gave varied results. According to Osann's formula, thirty-two grains of starch are rubbed in a mortar, with the same quantity of cold water; three grains of iodide of potassium are then dissolved in four ounces of boiling water, and the solution is added to the starch and well incorporated with it. After boiling once more, the test is cooled and placed in a bottle for use. When reagent paper is required, slips of Swedish filtering paper are shaken up with the starch liquid, and then dried. This paper is instantly colored of a deep shade of blue when exposed to the ozonized air of a bottle containing phosphorus and a little water. Exposed during the night to the atmosphere, the coloration by ozone is very decided.

In the ordinary atmospheric ozone observations the velocity of the air-current which traverses the reagent paper influences the result by bringing a greater proportion of ozone upon the test in a given time. The determinations which have been hitherto made are very vague, unsatisfactory, and yield but rude comparative results as to the subject in question.

Notwithstanding this difficulty, theories have not been wanting as to the influence of ozone upon the health. The oxidizing action of ozone upon organic substances as shown by Gorup von Besanez, the phenomena of ozone carriers, such as oil of turpentine, platinum sponge, and the *blood corpuscules*, have facilitated the formation of such theories.

Dr. Smallwood, in a long series of observations upon the atmosphere of Canada during the prevalence of the cholera and at other times, favored the inference of a deficiency of ozone during the epidemic.

Dr. Moffatt concluded, from a large number of observations in England, that the ozone in the atmosphere plays an important part in controlling or preventing epidemics, which it effects by removing the infectious matter from the air. During the prevalence of the cholera at Newcastle, in 1853, this gas was at its minimum. From August 24 to September 11, 1854, when the disease was at its height in London, ozone was observed only once, and then in but small quantity. On the 10th September a south wind prevailed, by which the ozone was found to increase, and the cholera cases diminished. During the Crimean war the surgeons of the French army established the following facts:

1. In proportion as the ozonoscopic papers were more colored in the air, the more numerous were the sick taken to each of the hospitals.

2. When the temperature was higher, there were fewer sick and fewer deaths.

3. At the three observatories the curves designating the proportion of ozone were essentially the same.

4. The curves for temperature were also identical.

5. At observatory No. 1 the quantity of ozone was *inversely* proportional to the number of deaths, while in observatory No. 2 this proportion was a *direct* one.

Bineau detected ozone but seldom in the air of Lyons, although he observed it always in the air of the neighboring country. He attributes the deficiency in the city to the action of organic matter in the air.

Pless and Pierre discovered 0.02 millegramme of ozone in 255 litres of atmospheric air.

Zenger found in twelve experiments from 0.002 to 0.01 milligramme of this body in 100 litres of air.

R. Angus Smith ascertained that the air of Manchester did not react with ozonoscopic paper, nor was there any reaction in the country when the air had first traversed the city; which he attributes to the absorption of ozone by the products of combustion, (antozone?) arising from the large amount of coal burned in the city.

C. Kosmann ascertained that in Strasburg, and at a short distance from the city, there was more ozone in the air of the country than in that of the city. In the city, the ozone maximum occurred in the daytime; in the country, at night. He found also, by the action upon Schoenbein's test paper, that the green portions of all plants exhale ozone.

Mr. Carey Lea discovered that the growth of plants is retarded in an atmosphere strongly impregnated with ozone.

A. Poey found that in the city of Havana the ozone reaction diminished with the elevation, while in the country the reverse was observed. He also discovered that the moisture of the atmosphere influenced the amount of ozone.

I ascertained that the air of the public grounds in Washington yielded, at night, abundant evidence of ozone, while the atmosphere of the streets of the city, observed at the same time, indicated an absence of this gas.

Dr. W. B. Rogers found that the air passing over Boston was deprived of its ozone by the emanations from the city.

M. Hozeau, of Rouen, inferred, from a series of observations which extended over four years, that the atmospheric ozone is least in February, begins to increase in March, and reaches its maximum in May and June, after which month it diminishes to its minimum in February.

J. Boehm has given the results of four years of regular observations of ozone, made in the city and environs of Prague. His ozonoscopic paper was observed and changed at 7 a. m. and 7 p. m., the proportion of ozone being determined by the depth of shade of the color produced. He found that the ozone bears no relation to the relative humidity, rain or thunder storms, clouds, &c., but is intimately connected with the force and direction of the wind, which was generally from the west when the phenomenon of color was observed. The reason of this fact is the relative position of the observatory to the city of Prague, so that all easterly winds passed over a thickly-settled portion of the town, by the emanations of which they were deprived of their ozone.

In the country and suburbs, Boehm found ozone constantly present. Without expressing a decided opinion upon the relation of ozone to health, this physicist calls attention to the fact that in Koenigstadt, a place not particularly noted for its salubrity, he obtained as decided reactions of ozone as upon the healthiest mountain, and that the coloration of the test paper is more decided in Vienna, a city noted for its lung and typhoid fevers, than in Prague, which is ranked justly with the healthiest towns.

From what is known at present respecting "antozone," it would seem that this body plays an important part in depriving the atmosphere of ozone.

In judging from the reaction upon iodide of potassium starch paper, ozone is always in the free atmosphere in variable, but small, quantity. Such test papers are not blued when closed in a bottle, and scarcely so, if at all, in a room. By exposure to the external atmosphere the effect upon them is extremely variable. Sometimes in cities it is imperceptible. According to Osann, the coloration is greater by night than by day, and is more decided towards sunrise, at the time when the atmospheric moisture is precipitated. It is greater in winter than in summer, stronger with clouded than with clear sky, and is especially powerful with snow clouds. Every flake of snow falling upon the test paper produces a blue stain. By atmospheric electrical discharges ozone is, as might be expected, very prevalent. Schoenbein calls attention to the fact that the odor in the neighborhood of places struck by lightning is exactly that of ozone.

The whole subject of ozone, whether in its physical or physiological relations, is intensely interesting, and promises, when understood, to be the means of solving many problems in the sciences now so difficult to be comprehended. No field of research appears to promise so rich a harvest to the skilled and patient observer.

VEGETATION AND THE ATMOSPHERE.

BY J. JAMIN.

TRANSLATED FOR THE SMITHSONIAN INSTITUTION FROM THE "REVUE DES DEUX MONDES."

PERSONS who are not devoted to the physical sciences will, perhaps, pardon me if I take the liberty of recalling to them that the air in the midst of which animals and plants live is a mixture of two very different gases. The one, almost inert and without appreciable influence in the phenomena of nature, is called azote; the other, on the contrary, possesses the most active properties and performs the most important part in the support of life on the globe; this is oxygen. Among other properties it possesses that of uniting with carbon or charcoal, and while this union, or, to apply the scientific term, this combination is taking place a considerable quantity of heat and light is disengaged. The carbon is said to burn, and it was thought at first, without reflection, to be annihilated; it is, however, merely transformed into a gas which mingles with the atmosphere, from which the chemist can recover not only all the carbon which has been burned, but all the oxygen which united with it. In order to recall to the memory the origin and composition of this compound gas it has received the name of carbonic acid.

Wood, which is essentially composed of carbon and water, burns in the same manner as carbon, expelling the water in the form of vapor and transforming the carbon, by its union with the oxygen of the air, into carbonic acid. Fruit, herbs, bread, and all our aliments, having a chemical composition analogous to that of wood, may, like it, be burned in a furnace, and Lavoisier has taught us that the substance of these aliments undergo a real but slow combustion in the respiratory system of the animals which eat them. Every animal is therefore a furnace, every aliment a combustible; the oxygen of the air is absorbed in respiration, is replaced by carbonic acid, and the water ejected by the natural outlets or by exhalation.

Since carbonic acid is necessarily produced by animal life it must form an integral part of our atmosphere. Chemists, in effect, detect it there, but in the minute proportion of four or five parts of the acid in ten thousand of air. It is a gas which can neither support life nor combustion, since it is, on the contrary, the product of these processes. Hence all animals confined under glass bells, filled with air, rapidly exhaust the oxygen, replace it by carbonic acid, and soon die, not from a poisonous effect of the gas, but from a want of respiratory sustenance.

Having recalled these facts, I shall describe a celebrated experiment which vegetables themselves are continually performing in our midst without our having consciousness of it, though it is accomplished on an immense scale, and may be justly considered one of the most essential phenomena of the world; an experiment, moreover, so simple that any one may repeat it at pleasure. In order to success, it is necessary to take a healthy and fresh branch, in full foliage, of one of those aquatic plants which grow immersed in ponds or rivers; introduce it into a glass jar, which is then filled with spring water, or, still better, with what is called mineral water, which contains, as we know, a large proportion of free carbonic acid; having closed the mouth of the jar when

full, we invert it in a basin filled with water; if we then open the mouth the water will retain its elevation and continue to fill the inverted jar. The apparatus being thus arranged is to be carried to an open place where it can receive the direct rays of the sun. As soon as the light strikes the leaves of the immersed plant we see them become covered with a multitude of bubbles, which rapidly enlarge, unite and rise to the upper part of the jar, where they accumulate. Whenever the light is intercepted by the intervention of an opaque screen the disengagement of bubbles stops, and we can, at will, and even at a distance, by alternately intercepting the light and permitting it to strike the leaves, arrest or restore the production of the bubbles. At the end of some hours of continuous action the jar will be filled with gas, which resembles in ordinary appearance atmospheric air, but has not its properties, for if we introduce suddenly into the interior of the jar a small taper which has just been extinguished, but which still retains at the extremity of its wick a few glowing points, it again instantly kindles and continues to burn with unwonted brilliancy. The gas is not air, but oxygen. In this form and with aquatic plants the experiment is striking, because the production of the gas in this case is rapid, and we assist, as it were, at the birth of the oxygen. We can produce the same effect, perhaps less rapidly, with all plants; and in order not to change their ordinary condition we may expose them to the sun, under glass bells, previously filled with carbonic acid; after the lapse of a day the carbonic acid gas will have disappeared and its place be supplied with nearly pure oxygen. Whatever may be the plant, or whatever be the experimental process employed, the action remains always the same. The explanation of the fact is easy. The green part of the vegetable decomposes the carbonic acid, extracts the carbon, which it appropriates to itself, and abandons the oxygen to the atmosphere. In the dark, and during the night, the part performed is changed; then, instead of absorbing carbonic acid, the plant gives it off; but the nocturnal reaction being inferior to that of the day, the plant performs a part on the whole which is opposite to that of the animal—it absorbs the carbonic acid which the latter exhales, and returns to the atmosphere the oxygen which the animal consumed.

Seeing the experiment so clearly, and its explanation so simple, it is difficult to conceive that they were not seen at the first glance. We find it difficult to believe that this was not the case; but every great discovery is made at a cost to humanity. At the beginning all is obscurity and perplexity, and it is only after long research and after much hesitation that we settle upon a few scattered truths, and when a clear and steady light illuminates all the previous obscurity it is only after the labors of several generations are collected and the efforts of a succession of men of genius have been devoted to the object. The history of great discovery is not without interest, and I purpose in the following pages to retrace the several steps which have led to the establishment of the relations which exist between plants and the atmosphere, including in the sketch some of the results of the more recent investigations.

I.

Charles Bonnet, a Genevese physician, was the first who experimentally undertook, about the middle of the eighteenth century, the problem which occupies us. It was the perusal of a once celebrated work, *The Spectacle of Nature*, which had decided his vocation. His attention was first directed to the subject of spontaneous generation, a question agitated even at that period, and the interest of which has but augmented with the progress of time. He relinquished this subject to consider another whose fertility he did not perhaps altogether anticipate; it was the inquiry, Of what use are leaves? and he made two experiments which have acquired a certain classic character. By the first, he proved that

light exerts over the green portions of vegetables so strong an attraction that, if placed in the dark, they direct themselves towards the smallest openings which convey it to them. The second showed that when plunged in water, plants disengage in sunlight a large quantity of *air;* but there Bonnet stopped: he knew not what that air was, nor could he know it, since at that epoch the first principles of modern chemistry were known to none.

Priestley, who was the rival and in some respects the predecessor of Lavoisier, was led by the results of his discoveries to study the action of plants on the atmosphere. He had just succeeded in isolating the remarkable gas which energetically supports the combustion of a lamp and the respiration of animals, and had for this reason called it *vital air.* He had ascertained, moreover, that small animals confined in this air or in atmospheric air soon changed its properties to such an extent that the animals died and the flame was extinguished. It is true, Priestley did not know the real nature of oxygen, and through a blind feeling of rivalry refused to the end to adopt the theory of respiration just announced by Lavoisier; but he knew, nevertheless, how to deduce from his experiments a logical consequence which was of the greatest importance. Perceiving that these little animals vitiated the confined air by their exhalations, he comprehended that all the individuals of the animal kingdom produce the same effect continually on the entire atmosphere, and that they must infallibly die, if there were not in the action of natural forces some inverse action constantly tending to restore to the air its purity, in proportion as this was destroyed by animal respiration. He proceeded to seek for this counterpoise, this regenerative action, and he found it in vegetables.

He placed in the air confined under a glass bell an animal and a plant. The former corrupted the air and died; but after the lapse of a certain time, Priestley discovered that the latter had restored to the air the vital property or the purity necessary to support life. This was one of the most important facts of the mechanism of our world. From this moment, it was known, though not yet in its details, that vegetables and animals execute antagonistic functions, these rendering the air unfit for the support of life, those repairing the mischief. The Royal Society of London conferred on Priestley, in 1773, the Copley medal, and in presenting it, the president of that celebrated company thus characterized the discovery of Priestley:

> "Plants do not grow in vain; each individual in the vegetable kingdom, from the oak of the forest to the grass of the field, is useful to the human race. All plants contribute to maintain our atmosphere in the degree of purity necessary to animal life. The forests, even, of the most remote countries contribute to our preservation, while deriving nourishment from the exhalations of our bodies which have become injurious to ourselves."

This glory of Priestley, however, was to be overclouded. After such successful labors, such grand and comprehensive views, such rewards and public honors, Priestley desired to repeat his former experiments, and obtained wholly opposite results; plants, instead of purifying the air, now appeared to him to pollute it. Surprised at this inexplicable contradiction between the past and the present, he multiplied and varied his experiments, and all that he could substantiate was, that vegetables possess the property at one time of purifying, at another of vitiating the air. The law which had won for him the Copley medal was therefore not general, and the consequences he had drawn from it not incontestable. A refugee in America, after a life agitated by religious discussions, Priestley died in 1804, having made in chemistry brilliant discoveries which he did not comprehend, and in vegetable physiology contradictory experiments which he was not able to reconcile.

Yet Priestley was deceived in nothing; plants do in fact alternately perform the two functions which he had assigned to them, and the only thing which he had not discovered was the condition which determines, frequently the restorative, occasionally the deleterious action, a condition which Bonnet had caught

sight of, and which Ingenhousz was soon to place in open day. Ingenhousz was born at Breda, in 1730; he was a physician, and came to England to observe the process of inoculation for the small-pox, which was then beginning to be practiced. It was during this visit that he became acquainted with the labors of Priestley, and resolved to explain their contradictions; this he succeeded in doing in 1779, and he has himself recorded his discovery in these words:

"Scarcely had I entered upon these researches, when the most interesting views presented themselves. I observed that plants not only possess the faculty of correcting impure air in six or more days, as the experiments of M. Priestley seem to indicate, but that they accomplish this important office, in the most complete manner, in the course of a few hours; that this surprising operation is by no means owing to vegetation, but to the influence of the light of the sun upon the plants; that it commences only after the sun has for some time risen above the horizon, and that it is completely suspended during the darkness of the night; that plants shaded by tall buildings, or by other plants, do not perform this function—that is, they do not purify the air, but, on the contrary, exhale a deleterious air, and diffuse a real poison through the atmosphere which surrounds us; that the production of healthy air grows languid towards the close of day, and entirely ceases at sunset; that all plants corrupt the ambient atmosphere during the night; that all parts of the plant are not engaged in purifying the air, but only the leaves and green branches; that bitter, ill-smelling, and even poisonous plants perform this office equally with those which diffuse the sweetest scents and are most salutary, &c." *

Ingenhousz had thus succeeded in discovering the force which occasions the respiration of plants. That force which had not before been suspected is from the sun, is *light*. It diffuses itself over the leaves, which absorb it, and fulfils the vast work of regenerating the atmosphere. Thus far the most important, as also the most difficult part of the task was achieved; but there remained yet quite as much to be done. The sciences resemble the sieve of the Danaïdes; each one tries to fill it; no one succeeds, because every discovery discloses a new horizon and presents a more remote goal which is never attained. After Ingenhousz, it still remained to be asked in what consisted that alteration of the air which animals occasion, and the remedy which vegetables supply. It belonged to chemistry to answer, and Lavoisier, though not specially operating with that view, furnished the solution of this new problem. He furnished it on that day when he showed that animals absorb oxygen, burn slowly the organic materials with which they are nourished and return by expiration a quantity of carbonic acid containing all the carbon which they have consumed. The vitiated or corrupted air, as Priestley and Ingenhousz called it, was consequently air deprived of oxygen and charged with carbonic acid, and, since plants purify it, this clearly indicates that they decompose that carbonic acid, retaining the carbon and restoring the oxygen to the atmosphere.

Considering the then existing state of chemistry, it might be thought that every one would have divined and proclaimed this explanation. It was not so, however, and new experiments were needed to discover it. It was a Genevese who had commenced this long train of deductions, and it was another Genevese who had the honor of completing it. His name was Sennebier; he had been the friend of Charles Bonnet, and to his example owed pursuit of the sciences as well as the councils which determined him to the study of the relations of plants and the air. He ascertained that vegetables placed in boiled water disengage no gas in the sun, but that they develop oxygen in abundance when the water has been previously charged with carbonic acid. He thence concluded that this gas is necessary to the respiration of plants, that it is decomposed by them, and thus had the honor of announcing the law already prepared and discovered by his predecessors. The question might have now been justly considered as solved; but during these labors, which had occupied more than half a century, many errors had become mingled with the truths obtained, and contradictory assertions threw doubt upon different points of detail. A review of

* *Experiences sur les Vegetaux*, par. 1, Ingenhousz, 1780.

all these phenomena was necessary; it was Thomas de Saussure who undertook to supply this, and who, without adding any capital fact to the cluster of previous observations, succeeded in giving them an experimental confirmation which has not since been contested. After these celebrated experiments, there was a long period of rest. Physicists and naturalists seem to have considered the question as exhausted, and transferred their inquiries to subjects which they thought more fertile. Nevertheless, the more recent labors of MM. Daubeny, Draper, Cloës, and Gratiolet, and above all of M. Boussingault, have successively intervened to raise difficulties which to this day remain in suspense; but I prefer to leave out of view whatever does not possess the interest of the general theory, and shall speak neither of the azote which vegetables seem always to disengage at the same time with oxygen, nor of certain deleterious gases, such as the oxide of carbon and carbonated hydrogen, which M. Boussingault has recently detected among the products of their exhalations; nor, lastly, of the attempts made without much success to appreciate the special influence of the different solar rays. What I wish to show is, that after the first investigations above recounted, we find ourselves confronted by a second class, far more extensive and complex, with which it is now necessary to deal. It is necessary to inquire what becomes of the carbon which remains in vegetables after the decomposition of carbonic acid.

II.

While the atmosphere furnishes carbon to the leaves, the boughs bring them water, which has been drawn from the soil, and it is natural to think that these two bodies, in meeting, pass mutually into combination; they, in effect, do combine and in very variable proportions; we will cite some examples: if 12 molecules of carbon unite with 20 molecules of water they are enabled to form either cellulose, which constitutes at once the vessels and entire skeleton of the plant, or fecula, which is known by everybody, or lastly dextrine, which is soluble and of which sirups are sometimes made; but, according to circumstances and the organs, the proportion of the two bodies may change, and with it the chemical products which take place. Thus 12 molecules of carbon combined with 14 molecules of water constitute glucose, or the sugar contained in ripe grapes; and if from this glucose we retrench two molecules of water, it is the sugar of the cane or the beet which would be formed. In fine, by processes which are unknown to us, water and carbon meeting in the leaves combine chemically, and produce an infinite number of compounds, differing according to the place, the organs, the nature, the age and the external conditions of the vegetable.

Besides the substances just spoken of, and which are compounds of carbon and water, plants give rise to still another class of substances which are characterized by an excess of hydrogen. These are the gums, oils, wax, balsams, essences, &c. Whence comes this hydrogen? They form also substances in which a fourth element, azote, makes its appearance. Does this come from the atmosphere? is it derived from the humus? These are questions which directly concern agriculture and for which it must consult chemistry. M. Boussingault is he who has treated of them first and best, and he has been placed in the most favorable circumstances for doing so, being at the same time at the head of a great agricultural enterprise, and habituated to the most delicate procedures of chemical analysis. The method he employs is proper to himself; it is sufficiently general and flexible to adapt itself to the demands of all special cases. It consists as follows: In a soil previously analyzed is sown a small number of grains whose chemical composition has been determined, and pure water is applied. This last disappears almost wholly by evaporation, and a small portion only becomes fixed. The plant grows, gains in weight,

because it draws nourishment from the air, and also because it seeks some in the soil. At the end of a certain period of vegetation it is gathered, and then we ascertain by new chemical analyses, first, how much carbon, oxygen, hydrogen, and azote it has gained; secondly, how much of those substances the soil has lost—that is, how much it has given up to the plant. The difference is due either to the air or to the water. That settles the account, and eventually the balance of profits and losses.

The application of this method, as rigorous in its conception as difficult in its practice, has disclosed a first fact of the same order as the decomposition of carbonic acid. All the plants have acquired an excess of hydrogen which comes to them not from the soil or the air, neither of which contains any; it has, therefore, of necessity, been derived from the water. Plants, then, do not limit their action to separating oxygen from carbon; they also divorce hydrogen and oxygen, retaining the first, expelling the second. The water was hydrogen burned, as the carbonic acid was carbon consumed; in both cases the plants have destroyed the effects of the combustion by delivering up the combustible bodies in the state in which they were before they were burnt. In verifying this action, finally exerted on the water, it has not been ascertained when it is effected or in what organs accomplished.

A second consequence flows from the analyses of M. Boussingault, namely, that every plant arrived at maturity has gained azote, which is deposited chiefly in its seeds; and as this azote may come either from the air which contains it in a free state, or from manures which have communicated it to the soil, it was necessary to institute special experiments to determine its origin. M. Boussingault proceeded as follows: he first sowed trefoil (clover) in a soil formed exclusively of calcined sand, which could only furnish to the plant mineral substances and the pure water with which it was moistened; as to azote, it contained none. Under these exceptional conditions the trefoil still completed all the phases of its vegetation, and in the end it had acquired a small but positive proportion of azote, which necessarily came from the air. The Jerusalem artichoke gave the same result with greater distinctness. After having matured, it contained twice as much azote as the seed from which it sprang; but when the attempt was made to reproduce the experiment with cereals, and above all with wheat, it was found that the azote of the grain was carefully preserved, but had in no degree augmented.

In all these cases the vegetation of the plants was extremely embarrassed, none of them having the aspect of healthiness which is witnessed in rich soils; the artichoke, however, suffered less than the trefoil, and this less than the wheat, which could not advance so far as to mature its grains. The reason of this is evident—azote was wanting; all plants need it, the cereals exact it, and when they do not find it in the soil they languish and often die. In order to confirm this conclusion M. Boussingault submitted to a comparative trial three plants of *helianthus* placed in three exactly similar pots, filled with pure sand and moistened with pure water. The first received no manuring, but to the second were supplied eight centigrammes, and to the third sixteen centigrammes of azotate of potash. From the first days the plants exhibited the difference of the treatment to which they were subjected. The first languished and died; the second vegetated, but remained sickly; the third was remarkable for its fine health. At maturity, the second had borrowed from the soil four centigrammes of azotate of potash, and the third eight. But what was chiefly remarkable was, that during its progress the last decomposed twice as much carbonic acid as the second. The azote thus performed the part of exciting the other functions and of giving to the subject which receives it, or of taking away from that which is deprived of it, the vitality without which it could not act upon the atmosphere.

Now, be it remarked, a plant contains more than the half of its weight of carbon and only some thousandths of azote. For what, then, does this substance serve in vegetation which is so necessary to it, although introduced in so weak a proportion? M. Payen will teach us. According to this skilful chemist all the organs of vegetation commence by an azotized matter analogous to fibrine, to which are added by little and little the cellular and fibrous tissues which in expanding produce the entire plant. This fibrine is never destroyed; is found in all the organs and is thus the rudiment of all the parts of the plant which cannot be developed without it, and consequently without the azote which is its essential base. In fine, plants are composed of carbon, water and hydrogen in excess; they contain besides a fourth simple body, azote, which occurs in a very minute proportion, but whose presence is essential to life. The atmosphere furnishes carbon in abundance; water, that is to say oxygen and hydrogen, is given by the rains; azote is required from the soil, and as it is rare therein we introduce it under the form of manure. It is the great care of the agriculturist; it is the heaviest, the most indispensable, and the most productive of his expenditures.

III.

Notwithstanding the important knowledge which we possess on the subject under consideration, it is still impossible not to recognize on many points the insufficiency of our information. That which is most inexplicable in our world, that which should most awaken our curiosity and invite our researches, is the great physiological fact of which I have related the discovery. The chemists have assiduously studied carbonic acid; they know all the properties it possesses; they can foresee all the reactions which it occasions or undergoes in the conditions in which it pleases them to place it; they are ignorant of none of the circumstances which produce it or destroy it; but they have never seen it steadily decomposing under the influence of light in the presence of some inorganic matter, and yet, what they cannot effect, the smallest leaf shone upon by the sun produces instantly with a rapidity and abundance which the naturalist regards with admiration. In ten hours an aquatic plant yields fifteen times its own volume of oxygen; a single leaf of the water-lily diffuses 300 litres during each summer; and M. Boussingault having directed into a vase filled with vine leaves, in the sun, a current of carbonic acid, received on its exit only pure oxygen. Now we are obliged to acknowledge that this fact, so common, so easily accomplished by the leaves at every hour of the day, chemistry can neither comprehend nor imitate.

If we cannot understand and imitate the conditions of a fact relatively so simple and so definite, what must not be our embarrassment when we would analyze the chemical and physiological phenomena which ensue from it? We see in effect three simple bodies, and rarely four, combine in relations indefinitely variable in order to give rise to the most numerous and different compounds—wood, starch, sugars, oils, wax, balsams, essences, both fragrant and offensive, delicious fruits and violent poisons, acids like vinegar, and alkalis like quinine or strychnine, coloring or colorless substances—in a word, products whose infinite variety transcends the dreams of imagination. Not without dismay must we measure the depth of our ignorance in the presence of phenomena so multiplied, and whose mechanism escapes us so absolutely.

There are, however, ill-disciplined minds which wish to explain everything, especially what they are most ignorant of. It has been said that plants probably contained compounds of carbonic acid and of azote, formed at night and decomposed in the light during the day; it has been also said that there exists in green leaves a sort of fermentation deriving its activity from the sun, and

whose function it is to decompose carbonic acid. These explanations have not only the defect of being illusory and conjectural, they are demonstrably false, for the pounded leaves, which preserve the same composition, ought then to continue the same functions, which is not true. There is also a whole school of naturalists who content themselves with ascribing the functions of vegetables to what they call life—a kind of unapproachable force which suffices to explain everything by the sole virtue of its name; these appear to me to renounce all of scientific progress, like the ignorant devotees who explain all phenomena by saying that God produces them. It is God, beyond a doubt, who has ordered the world, but he permits us sometimes to inspect the mechanism. Without doubt, also, it is life which disposes the functions of beings, but before proposing it as the final cause and ultimate explanation of facts, it behooves to know a little what life is, and of what contrivances it makes use. We see to what weakness we are reduced as soon as the basis of experiment fails us, when, in order to fill up the gaps of our knowledge, we strive to grapple ourselves to hypotheses, to unexplained forces which explain nothing. Let us honestly avow our ignorance, and gird up our loins and seek.

To console ourselves for this avowal, which may possibly hurt our self-love, and to find encouragement for the labors of to-morrow, let us measure, with a view to their consequences, the importance of the facts which we know to-day. If plants give out oxygen, animals absorb it, and a compensation is established between these inverse functions. We can demonstrate it experimentally by confining under a glass bell an animal and a plant. Separated, each of them would die—the first by being suffocated in the carbonic acid it exhales, the second because it would be deprived of this gas which nourishes it. United in the dark, the animal and the vegetable would injure instead of aiding one another; but in the light of the sun the life of the one supports that of the other; the animal, burning his aliments, furnishes carbonic acid to the plant, and the latter restores to the animal the oxygen which is necessary to it. This experiment would be in little the image of the world, and it is thus that Priestley conceived the eternal equilibrium of it. Nothing can be more grand and beautiful than this thought, but it is necessary to complete it. If the bell of which we have just spoken were very small, the least excess which might occur in the respiration of the animal, or the least interruption in the action of the sun, would exaggerate the quantity of carbonic acid and cause first the animal to perish, and then the vegetable. Are we, then, exposed on the earth to a like danger, and are plants so necessary to us that we must cease to live as soon as they should cease to act? Believe it not, for we shall demonstrate that this fear would be vain. The human population of the globe may be approximately rated at a thousand millions of individuals, and we shall not be far from the truth in assuming that all other animals taken together exert upon the atmosphere, by their respiration, an effect equal to that of three thousand millions of adult men. This makes for the whole animal kingdom a population equivalent to four thousand millions of human beings. Now, as the mean quantity of oxygen which an adult human being consumes in a day has been measured, we can calculate that of the total population of the globe. It is very great, no doubt, but, on the other hand, the provision of oxygen in the atmosphere is greater still. It is so much greater than the consumption of animals that it would require eight thousand millions of years to exhaust it. In eight centuries it would fail but a millionth part, and if the vegetables ceased their action, it would require at least two thousand years before the nicest chemical analysis could avail to detect a change in the composition of the air. The service that vegetables render us is therefore much less immediate than Priestley thought; it is a service of distant reversion, and we may without ingratitude relegate our acknowledgments to posterity.

But the earth is quite old, and it is not impossible that its atmosphere has undergone, since the creation, progressive changes which have become very considerable through the lapse of so many ages. We have here a very curious question, which has been considered by M. Brougniart, and which we will proceed to study with him. The earth covers enormous, we might say inexhaustible, masses of carbon under the form of coal, anthracite, lignite and peat, and it cannot be doubted for an instant that these deposits are not the accumulated fossil remains of innumerable vegetables. Now there is for a plant but one single mode of acquiring carbon—to imbibe it in the form of carbonic acid from the air, and consequently all those masses of coal which cover Belgium, England, a large portion of America, and which are found at all points of the globe, were once diffused in a gaseous state through the atmosphere; they were there combined with oxygen, and the globe in the beginning was involved in an aeriform envelope which contained azote, a great deal of carbonic acid, little or no oxygen. If we add that, at the moment, the earth was incandescent, we see that all the carbon must in effect at that temperature have been burned on contact with oxygen.

Thus constituted, the earth cooled down; but the composition of its atmosphere rendered it uninhabitable for animals, since they had need of oxygen and there was none, since they would have been suffocated in the carbonic acid and azote which prevailed at the moment. Hence the first strata of sedimentary deposits contain no animals. In return, the earth was as favorably prepared for the production of plants as it was unfit for the nourishment of animals; it was soon, therefore, covered with luxuriant forests, whose remains, in accumulating, formed coal. We find therein all the species then living. There were gigantic *equisetums*, arborescent ferns comparable to our oaks, and palms which towered above everything that the vegetable kingdom now offers us. And while these immense deposits were forming, oxygen, perpetually disengaged by the action of the sun, was gradually impregnating the atmosphere and preparing it for the advent of the animal tribes. Of these, in due time, the first creations made their appearance, having since varied from age to age. At the epoch of the coal formations the forests were tenanted by huge reptiles, cold-blooded animals, for which little oxygen sufficed; but it was not till after the nearly total disappearance of the carbonic acid that the earth witnessed the arrival of the mammifers, which had awaited a richer atmosphere.

There are those at once timorous and ignorant who seriously ask what will become of the earth and themselves when mankind have burnt up all the coal. I will tell you, honest folks, what will become of us. The coal will have again been converted into carbonic acid, oxygen will have disappeared, and the great vegetable tribes will return; but if it is true, as they would persuade us, that the animal species, by growing gradually more perfect, have advanced from the primitive forms up to man, the return of the elements to their point of departure would bring man back to his origin by an inverted degenerescence. To have had crocodiles among our ancestors, be it so; but to see in perspective a posterity composed of ichthyosauri, this certainly is the most disheartening of metempsychoses!

But to return to serious matters: if we are ignorant of the mechanism of the living organs, at least we know the functions they fulfil, and can express clearly the part which they play in the physical world. With the water and azotized substances which they take from the soil, with a gas which they collect in the air, vegetables compose organic matter, which they accumulate in their tissues and which they hold in reserve for the use of animals. The vegetable kingdom seems to be only a great laboratory, an *atelier* of production where every plant has the same function—that of forming materials as varied in their composition as are the forms of each one of them. To this common

character it is necessary to add another, which is that receiving as primary material carbonic acid and water, substances burnt, plants have the faculty of expelling the oxygen and of extracting from them the carbon and hydrogen, to which they restore the property of being susceptible of being burnt anew. These chemical actions take place within their organs, but these organs are only the seat of them; the cause of these actions is without; it proceeds from the sun.

The animal has received a diametrically opposite mission. It creates not, it destroys; in place of solidifying the liquids and gases, it separates them and restores them to the atmosphere; in fine, far from bringing back bodies to the combustible state, it burns them. The herbivorous animal derives all his nourishment from plants; he transforms a part of them into water and carbonic acid, he accumulates the rest in appropriate organs. The carnivorous profits of these reserves, and finishes by restoring to the atmosphere what vegetables have extracted from it; what the herbivores have preserved of it, and whatever the class to which it appertains, every animal rejects by the natural ducts an abundant provision of azotized matter which it deposits on the soil. It is precisely this matter which vegetables take up again, without which they cannot live, which they possess the power of elaborating, transforming, accumulating, and which they return to animals after having restored to it the nutritive qualities which it had lost. Thus is closed that admirable circle of opposite transformations and of mutual services where we see the animal and the vegetable eternally exchange the same matter; this, which receives it gaseous, disoxidizing and solidifying it; that, which receives it combustible, again dispersing anew after having burnt it. Priestley saw in plants predestined servitors whose office it is to purify the air; but they have another function much more immediate and render us a service quite otherwise indispensable, that, namely, of extracting and preparing our aliments. Their action on the air would only be sensible after a long succession of ages; but if a single year drought annihilated the fruits of the earth, a frightful famine would destroy in a short time all the animals which the globe nourishes.

From the sun it is that daily nourishment, life, force, and all our power is derived. The light, the chemical emanations, all the rays which that orb sends us, are extremely rapid vibrations, analogous to those produced by sound; there is movement, there is force; as soon as it reaches the plant that force is absorbed, it disappears, it is extinguished. But no force is extinguished except on the condition of having produced an effect, performed a work which is its equivalent. Now the work performed by the light which the leaves absorb is decomposing the carbonic acid. So, too, let it not be forgotten, there is needed a given amount of force to disunite a given quantity of oxygen and carbon; it is the sun which every hour of the day furnishes it gratuitously.

If now we place in presence of one another this oxygen and carbon, and, by an inverse operation, combine them by burning this carbon, they will produce, in uniting anew, all the force which it had been necessary to expend in order to separate them; that is to say, all which the sun had furnished. There will be heat and light, as experience shows, and there will be force also, which may be collected by means of calorific machinery and employed in our service. And we shall do well to reflect that it is the sun which has prepared for us that heat, that light, and that force; which has furnished to the carboniferous forests at an epoch when man as yet was not, what man recovers and disposes of to-day.

And what is true of our inanimate furnaces will be found to be repeated in those living furnaces which we call animals. They likewise burn organic material, produce heat which elevates their temperature, and develop force and movement: a force which they do not create, which they owe to that very com-

bustion, and upon the same terms as do steam-machines; a force previously infused by the sun into plants, absorbed by them, virtually preserved in their products which are our sustenance, which we disengage by respiration and which our muscles apply under the direction of our wants and our will. This whole grand generalization of the phenomena of the world is the work of modern chemists and physicists. It was MM. Dumas and Boussingault who first disengaged it; the mechanical theory of heat completed and demonstrated it; but it already existed entire in the conception of Lavoisier when he wrote:

"Organization, spontaneous movement, life, exist only on the surface of the earth in places exposed to the light. It might be said that the fable of the torch of Prometheus was the expression of a philosophical truth which had not escaped the ancients. Without light nature would be without life—it would be dead and inanimate. A beneficent God, in supplying light, has spread over the surface of the earth organization, sentiment, and thought."

IV.

If during the regular course of its existence, a vegetable accumulates organic matter, there are nevertheless two moments when it loses this essential character and comports itself like the animals: it is at the commencement and the end of its life, when it germinates and when it reproduces itself. Every seed, besides the embryo which for long years preserves the principle of life, encloses a provision of organic matter destined for the first nourishment of the springing plant. Cast on a warm and humid soil, it germinates; its radicle seeks in the soil a point of support and liquids; the germen rises upward; the seminal leaves or cotyledons are developed, and the rudimentary plant is established in virtue of intrinsic and transmitted life. Now, during this first period, the provision of accumulated matter is divided into two parts: one is burnt by a sort of respiration, the other, undergoing complicated chemical actions, is transported into the organs and there becomes fixed in constituting them. Everything occurs nearly as in an animal and without any intervention of light; but after this primitive phase, when the respiratory organs have received their first development, the plant waits for the rays of the sun to continue its evolution, and, as soon as these reach it, it inclines towards them as if eagerly to collect them, it becomes green, and commences, only to desist at its death, that decomposition of carbonic acid and that accumulation of matter which is its function and its predestination.

In order better to study this period of intrinsic life in the seed, M. Boussingault conceived the happy idea of prolonging it by indefinitely retarding the action of the light. The experiment was made with peas, in a soil without manure. After having germinated, they continued to grow, giving forth a pale, slender, creeping stem which at length perished without having borne seeds. During this whole period the peas continued to work up the organic material originally contained in the seed, and in proportion as their life was laboriously prolonged, they dispensed it by little and little in order to sustain it. At last, each plant had lost more than half the carbon which the seed had originally provided. While this experiment was going on in darkness, other peas, sowed at the same time, were successively transferred into the light. From that moment everything became changed; real life was developed, and the plant, being now able to avail itself of the nourishment contained in the air, gained each day, in the sun, very nearly as much carbon as it had previously consumed in darkness.

In nature all things touch upon one another: vegetables in the seed, animals in the egg, appear to accomplish the same acts and exist in the same conditions. In both cases, a mass of organic matter accompanies the germ; the egg and the seed may preserve for an indeterminate length of time the virtual principle of life. A little heat will commence the evolution, and from that moment the

organic matter, absorbed by the nascent tissues, transported by the vessels which are forming, takes its place in the organs into whose constitution it enters. During this whole period, the plant and animal subsist on their own resources, drawing nothing from without, and to complete the analogy, they burn a portion of their own substance. By and by, when all is exhausted, the animal, already formed, is prepared to live, as the plant, already delineated, is prepared to vegetate, and a common want displays itself at the same moment in both existences: that of finding external nourishment. From this point all analogy ceases, and the separation of the two kingdoms commences. The vegetable creates and reduces, the animal destroys and oxydizes.

Let us pursue these analogies. In every flower that opens, botany of late has shown us the organs of two opposite sexes which concur, each after its character, in the fecundation of the germs. Now, at this moment, when the flower seems to borrow that sexual function of reproduction which we might think to be the exclusive privilege of animals, it again imitates them in burning the organic material by an active respiration. "All flowers," said Priestley, "invariably exhale a deadly air during the day and during the night, in light and in darkness." Daily experience confirms this assertion, and De Saussure has shown that this poisonous gas is carbonic acid. At last one of our most justly celebrated chemists, M. Cahours, has given us the results of a recent and complete study of this respiration of flowers and fruits under all circumstances.

If it is true that this combustion of organic matter, that this expenditure and loss of force, be necessary in itself to accomplish the act of fecundation, it is in the sexual organs especially that it should be present. Experiment in effect has confirmed this conjecture, and it has even been ascertained that it is the stamen, the male organ, which dispenses the most. Nor does this fact stop there. All combustion disengages heat: it is to their respiration that animals owe their high temperature, and it is of course necessary that the stamens and pistils should develop heat since they respire. The question was to find thermometers sufficiently sensitive and a suitable plant. The first vegetable which has allowed the verification of a rise of temperature is one which would never have been suspected of so much ardor, the pumpkin. Its flowers are large, and admit of the introduction of the air-thermometer; some of them are male and others female, and the latter have evinced a greater degree of coolness than the former.

Still the gourds, melons, and pumpkins grow warm in but a slight degree, and so, it might be said, resemble the cold-blooded animals. There are plants which resemble the warm-blooded animals, and these are the Arums. One of them, the *Arum maculatum*, which is found abundant in hedges, is enveloped in a rolled leaf which encloses the flower in a chamber, and which prevents the heat from being dissipated in space. Observe now the singular phenomenon which has been perceived by Lamarck, Sennebier, Bory de Saint Vincent, and by De Saussure himself. Habitually the Arum is cold, but at a given moment, which must be watched and skilfully improved, the temperature of the plant raised from 7 to 8 degrees above that of the atmosphere. Hubert, a truly sagacious observer, succeeded in introducing a small and very sensitive thermometer, sometimes among the stamens, which became heated to 22 degrees, sometimes among the pistils, which produced an action one-half less. The other parts of the plant manifested no special action. By care and watchfulness, De Saussure surprised four Arums at the moment of calefaction, and placed them under a glass bell filled with air. The glass was immediately covered with a moisture which attached itself to the surface, a great absorption of oxygen took place, and a correspondent production of carbonic acid. In its chemical action and the energy of that action, the plant was comparable to a small animal. At another time De Saussure decomposed the plant into different parts, which he

studied separately: the sexual organs consumed 132 measures of oxygen, and the rest of the flower only 30.

After fecundation, the fruit begins to be developed and the plant to nourish it. Not only does the plant furnish it with the matter accumulated in its own tissues, but with a quantity greater still, which it burns by a respiration proper to it. The whole life of the vegetable seems then exclusively devoted to the accomplishment of this last duty of nourishing the fruit. In this task it impoverishes itself; the beet and the cane dispense all the sugar they possessed, every plant exhausts the provisions which it had accumulated in the period of its youth, and when the fruit is mature, the vegetable, if it is annual, is reduced to a dry skeleton, and if it is perennial, sinks into the repose of winter, to recover its forces and recommence, the following year, its providential function.

The subject under consideration, besides the questions of detail which I proposed to examine, contains a great truth with which I shall conclude, namely, that our world does not suffice for itself, because it is deficient in force; but it receives this from the sun, diffused upon it in the form of rays. And it is by virtue of this action that life on the globe is transmitted under two antagonistic forms—vegetable life, which accumulates force by creating organic matter, and animal life, which consumes and dissipates that which the sun furnishes, that which vegetables absorb and treasure up.

EXTRACT OF A MEMOIR

ON THE

PRESERVATION OF COPPER AND IRON IN SALT WATER.

BY M. BECQUEREL.

BUREAU OF NAVIGATION, NAVY DEPARTMENT,
Washington, April 20, 1865.

DEAR SIR: The protection of the bottoms of iron vessels from corrosion by sea water, and from fouling with animal and vegetable matter, is one of the subjects referred by this Department to the National Academy of Sciences for examination and report. I need not enlarge upon its paramount importance. A memoir by M Becquerel, on this subject, in the first number of the fifty-ninth volume of the *Comptes Rendus*, has recently attracted my notice, and I think the publication of it in English may be serviceable by calling the attention of professed chemists and other systematic experimentalists to the strictly scientific treatment of the question.

I have the pleasure, therefore, to send you the accompanying translation, which I should be happy to see preserved and widely circulated in the pages of your valuable reports.

I have the honor to be, very respectfully, your obedient servant,

C. H. DAVIS,
Rear-Admiral, and Chief of Bureau of Navigation.

Prof. JOSEPH HENRY, LL. D.,
Secretary Smithsonian Institution, Washington, D. C.

THE preservation of metals at sea, especially of copper and iron, has in our time become a vital question on account of the transformation of the navies of all nations; a transformation suited to bring about a change in their mutual relations. Since this question falls within the province of the physico-chemical sciences, I have considered it my duty to give it particular attention, with the hope of adding by my own efforts some new data in aid of its solution to those which we already possess.

This question presents great difficulties, proceeding from numerous causes which contribute to the alteration of metals. All these causes, whether mechanical, physical, or chemical, exercise an influence over chemical action, and consequently over the production of electricity, which gives rise to isolated voltaic couples. They can only be effectually controlled by the closest investigation and by contending, so to speak, with each of them singly.

Finding it impossible to communicate the whole of my labors to the Academy, I shall confine myself to laying before it a concise abstract of the principal results of my investigations, in order that it may get an idea of their whole scope; but, before doing so, I will cite those results which have already been obtained on the same subject, and thus make the Academy acquainted with my point of departure.

In a lecture delivered January 22, 1824, before the Royal Society, (*Annales de Chimie et de Physique*, t. xxvi, p. 24,) Davy informed his audience that the rapid change in the copper-sheathing of vessels-of-war, and its unequal durability, had excited the particular attention of the lords of the admiralty, who employed him to investigate the means of preserving the sheathing; and that he immediately undertook a series of researches which led him to the discovery of an important principle, according to which a metal which is electro-positive in salt water, being converted into an electro-negative, is preserved from all alteration, at least within certain limits.

Davy admitted the theory of contact, that is, the production of electricity by the contact of two metals, resulting from mutual action. Chemical action, according to him, only served for the transmission of electricity from one body to another. This view prevented him from deducing from his discovery consequences which naturally flow from it. His first statement was, that a piece of zinc of the size of a pea, or of the point of an iron nail, was quite sufficient to preserve from 40 to 50 square inches of copper wherever placed, and that a little piece of zinc having been fixed on top of a piece of copper, and a much larger bit of iron below it, and the whole immersed in salt water, the copper not only was preserved on both sides, but the iron also, which, after a fortnight, had kept its brightness equally as well as the other metal. He concluded from this, at once, that small quantities of zinc, of iron, or of cast-iron, when placed in contact with the copper-sheathing of vessels, prevented its corrosion. He added, besides, that, since negative electricity could not be regarded as favorable to animal or vegetable life, because it caused the precipitation of magnesia upon copper, a substance very prejudicial to land plants, this electricity ought to help to keep the bottoms of vessels clean.

The lords of the admiralty having furnished him with the means of experimenting on a large scale with his mode of preserving the copper-sheathing of vessels at Chatham and Portsmouth, he established the following facts:* Sheets of copper in contact with zinc, iron or cast-iron, over $\frac{1}{40}$ or $\frac{1}{1000}$ part of their surface, having been exposed for several weeks in the harbor of Portsmouth to the action of the tide, and their weights having been determined before and after the experiment, Davy found that when the metallic protector covered a surface of from $\frac{1}{40}$ to $\frac{1}{150}$ of the copper, neither corrosion nor diminution of the latter metal took place, but when the ratio was from $\frac{1}{200}$ to $\frac{1}{400}$ the copper underwent a loss of weight greater in proportion as the protection was smaller. He considered cast-iron, a substance so readily and cheaply found everywhere, as the best and the most appropriate for the protection of copper, and as lasting as long as iron and zinc.

The sheets of copper of two small vessels, thus protected, were kept perfectly clean for several weeks, as long as the metallic surface of the copper had remained uncovered; but as soon as the metal was covered with carbonate of lime and magnesia, plants and insects collected there.

Again, we find the following facts in the Philosophical Transactions of London for 1825, pp. 340 *et seq.:*

"The first experiment of this kind was tried on the Sammarang, of 28 guns, in March, 1824, and which had been coppered three years before in India. When she came into dock at that time, before she was protected, she was covered with thick green carbonate and submuriate of copper, and with a number of long weeds, principally fuci, and a quantity of zoöphites, adhering to different parts of the bottom. For the purpose of protection, Davy employed cast-iron, equal in surface to about $\frac{1}{80}$ of that of the copper, which was applied in four masses, two near the stern, two on the bows. She made a voyage to Nova Scotia, and returned in January, 1825. When she was again brought into dock, there was not the smallest weed or shell-fish upon the whole of the bottom from a few feet round the stern-protectors to the lead on her bow. Round the stern-protectors there was a slight adhesion of rust of iron, and upon this there were some zoöphites of the capillary kind, of an inch and a half or two inches in length, and a number of minute barnacles, both *Lepas anatifera* and *Balanus tintinnabulum.* For a considerable space round the protectors, both on the stern and bow, the copper was bright; but the color became green towards the central parts of the ship; yet even here the rust or verdigris was a light powder, and only small in quantity, and did not adhere, or come off, in scales, and there had been evidently little copper lost in the voyage.

"The yacht Elizabeth was protected by about $\frac{1}{120}$ part of malleable iron placed in two masses in the stern. She had been occasionally employed in sailing, and had been sometimes in harbor, during six months. When Davy saw her, at the end of this time, she was perfectly clean, and the copper apparently untouched. Her owner informed him that there

* Philosophical Transactions, 1824; *Annales de Chimie et de Physique*, t. xxix, p. 187.

never had been the slightest adhesion of either weed or shell-fish to her copper, but that a few small barnacles had once appeared on the loose oxide of iron in the neighborhood of the protectors, which, however, were immediately and easily washed off.

"The Canebrea Castle, a large vessel of upwards of 650 tons, was furnished with four protectors, two on the stern and two on the bow, equal together to about $\frac{1}{107}$ of the surface of the copper. She had been protected more than twelve months, and had made a voyage to Calcutta and back. She came into the river perfectly bright, and when examined in the dry dock was found entirely free from any adhesion, and offered a beautiful and almost polished surface, and there seemed to be no greater wear of copper than could be accounted for from mechanical causes."

It follows from the preceding facts that, whether in sea water or the salt water of the laboratory, the copper sheets which are at rest in salt water increase in weight by becoming covered with earthy or alkaline deposits when they are protected by a proportion of iron below $\frac{1}{150}$; and if this proportion be contained between $\frac{1}{150}$ and $\frac{1}{1000}$, the surface seems to be preserved without receiving either deposits or zoöphytes or shells.

It appears that Davy devoted himself to determining the limits on the surface within which the protection takes place, but not at all in the thickness. He neither takes into account the layer of oxychloride of zinc nor of copper, oxychloride mixed with particles of one of these two metals, which, by opposing the reaction of salt water, stops the protection. The whole question lies there, and Davy studied only the theory of contact.

Although several of the preceding experiments have furnished favorable results, nevertheless the process of protection was not adopted; the reason assigned for it was the negative condition of copper, which favored the deposit of marine bodies to such an extent as to diminish the rate of sailing.

We shall see presently that the deposit of marine bodies was not to be attributed to this cause, since the major part of the protection had disappeared. It has been remarked, however, that in order to preserve the copper the protecting metal must be oxidized. He had such a strong belief in this theory that he asserted that a piece of zinc of the size of a pea, or the point of an iron nail, was sufficient to protect copper plates of 256 to 320 square centimetres of surface, immersed in sea water; this preservation could last only for a short period, as the piece of zinc, or the point of a small iron nail, was rapidly destroyed. There is, withal, in this nothing to inform us whether, in the means of preservation employed at sea, a thought was given to the disadvantages resulting from the destruction of the oxidable metal; it is not, therefore, astonishing that the copper-sheathing became foul, to use the seaman's phrase, and was covered with organic bodies. In addition to the above, it is the same with regard to the production of electricity for preserving copper or iron at sea as with regard to the production of heat; in the latter case it is necessary to keep up the supply of the combustible material, and in the former to provide for the replacement of the oxidable metal according as it is destroyed; this is an indispensable precaution to secure its preservation.

The causes of change in metals are numerous. We will cite particularly the heterogeneousness of parts: the difference in the mode of aggregation of particles, the presence of any bodies whatever on the surface of metals, of grains of sand for example, or spots of rust, strokes of the hammer falling here or there, pressure, a simple fold or corrugation, &c., are so many causes which give rise to voltaic couples on the surface, and which destroy a protection otherwise suitably selected. The friction of water must still be added, as M. Ed. Becquerel has shown in the interesting experiments which he repeated at Toulon, while aiding me in my investigations.

From this it may be seen why it is that metals like iron, that are forged, wrought, and hammered, present so many causes of change, which are removed by means of protectors, arranged according to electro-chemical principles. This explains, what experiment proves, why it is that the electrical condition of the protected metal does not always follow a regular law.

I have been guided in my researches by an important consideration, which I will here mention: it has been discovered, that in order to decompose one milligramme of water, it was necessary to employ in the form of a current a quantity of free electricity equal to that of 20,000 batteries, each of one square metre of surface, and charged in such a way as to give out sparks flying more than one centimetre in length. This is the quantity supposed to be combined with the substance and which becomes free on the decomposition of one milligramme of water, or which experiences some kind of transformation, either by becoming heat or by being changed into a vis-viva, of which I have tried to determine the effects in the cases under consideration. This much is certain, that but an exceedingly small portion of the enormous quantity of the electricity capable of producing the effects of thunder is collected; but before arriving at this determination I have investigated with the utmost minuteness, by means of the compass of sines and suitably arranged apparatus, the electro-motive force of zinc as well as that of iron, of copper, of lead, and their alloy, plunged in sea-water, which forces bear a relation to the developed affinities and naturally serve as starting points for finding the protecting metal or alloy; this latter acting only with efficiency when the negative condition of the protected metal, which is derived, is superior to that which it assumes when it is attacked by salt water. I subsequently determined the electrical condition of all the components of a protected metal in order to see what became of the vis-viva, of which I have just spoken, and to discover the laws upon which it will be necessary to rely to secure protection. My mode of proceeding was as follows:

When a copper plate 5 metres in length and 6 centimetres in width, containing a surface of 3,000 square centimetres, and armed at one of its extremities with a very small band of zinc one centimetre square, and furnished at short distances with vertical rods of the same metal and other details, is plunged into sea water, it will be found that from the zinc to the other extremity of the plate the electrical condition of each point of the latter gradually diminishes, and if the curve of the intensities be traced, by taking for the axis of the abscissas a line which represents the electro-motive force of copper, for the abscissas themselves the distances to the zinc, and for ordinates the corresponding electrical conditions, this curve will appear to have the axis of the abscissas for its asymptote, showing that it cannot be determined how far the protection extends. The whole surface of the copper retains its brightness, with the exception of the part on the side of the zinc to a distance of about one or one and a half metres, which becomes covered with earthy metallic deposits when the salt water is not pure. In another experiment made at sea, the law has been verified to about fourteen metres. It is evident, therefore, that in the oxidation of the zinc the electricity which ceases to be united with the substance, and which is enormous in amount, acts as a living force, when it is transmitted to the copper at distances the precise limits of which are not known. We must not forget here to observe that there are in circulation over the whole metallic surface, by means of the liquid which moistens it, derived currents which produce electro-chemical decompositions, and which are created at the expense of the electricity disengaged in the oxidation of the zinc.

Hence we perceive, that if we wish to protect a copper surface in such a manner as to avoid electro-chemical deposits, we must arm the surface with a metallic protector having an electro-motive force equal to that of the point where these deposits begin to be insensible; this is an important condition to fulfil in order to prevent deposits of shells and other marine bodies, which seem to be formed on parts already covered with limestone, magnesia, and other substances.

Copper plates armed with iron, and iron plates protected by zinc, present similar effects, with the slight difference that the sphere of electric action is less, it being understood that its extent depends on the difference between the electro-motive forces of the protecting and the protected metal.

One can hardly form an idea of the slight space which it is sufficient to give to the zinc and to the iron in order to produce on the metals which they are protecting the effects we have just mentioned; thus the quantity of metal needed to protect the iron of an armed vessel becomes insignificant.

The protecting alloys of zinc and copper, of zinc and lead, &c., act, in proportion to the more oxidable metal which enters into their composition, with certain conditions of hardness, to which regard is to be paid. With an alloy of copper and zinc, the protecting power diminishes, according as this last metal is oxidized and carried off, when there remains finally nothing but a copper sponge, which is soon changed into oxychloride; the greater the hardness of the alloy the slower the production of the effects here mentioned.

The experiments of which the principal results have been stated had to be repeated in the open sea. The minister of marine, fully appreciating their importance, was kind enough to put at my disposal in the harbor of Toulon all the necessary means for making these experiments. I cannot sufficiently thank him, as well as M. Dupuy de Lôme, the latter particularly, on account of the useful information which he so kindly furnished me in respect to what concerns the applications of my experiments. I also thank MM. the naval engineers for their co-operation, and M. de Mouy, sub-engineer, who, having followed my experiments with attention, will be able to repeat them. The experiments have been made on a large scale and have not left any doubt as to the accuracy of the results obtained in the laboratory, and have enabled me moreover to make new observations, which are of interest in applying the experiments. I must mention here some observations which ought to be taken into consideration.

Whenever the iron plating is covered with several coats of red lead, it is preserved as long as the paint lasts; but as soon as it is partially removed, either by friction or by the dissolving action of the sea, which is slow, the metal begins to be attacked at different places; those parts which have lost paint are negatived relatively to those which preserve less of it, or none at all; so that these last suffer more than the others. From the above causes spring those local changes scattered occasionally over the surface of the plating, which will be easily avoided by the employment of protectors, disposed according to principles here laid down—protectors which will not come into use until the paint is carried off.

The copper sheathing of the bottom which is not painted, being in the same condition as that of the old vessels, will be exposed to the same disadvantages, unless it be protected not only with a view to its preservation, but still more for preventing deposits of earths and other matters, which seem to favor deposits of shells, mollusks, and marine plants, which it is said do not occur as long as the surface continues bright.

All the parts constituting the sheathing and the armor have been so well adjusted by M. Dupuy de Lôme, that it will be quite easy without disturbing anything to apply the protectors in such a manner as to clean the former or change them as need be.

It will even be possible, when the vessel is on the point of leaving the basin to enter the harbor, with the aid of apparatus I constructed for this purpose, to see if all metallic parts are completely protected, or in case they are not, to discover the amount of change.

Such are the general results arrived at during the long investigations conducted either in the laboratory or in sea water upon the means to be used for preserving the metals employed for the plating and sheathing of iron-clads, and for preventing deposits of shells and other marine bodies.

It is quite impossible for me in this extract to enter into details concerning the measures to be taken for the preservation of metals, an account of which is given in this memoir; it is sufficient for me to say, that the general principles appear to be well established, and that the only questions still waiting their solution are those which relate to the application of these principles in detail.

PRESERVATION OF WOOD.

TRANSLATED FOR THE SMITHSONIAN INSTITUTION BY C. A. ALEXANDER, FROM THE LEIPZIG "AUS DER NATUR. U. S. W."

The increased consumption of wood, more especially in the construction of railroads, has rendered the question of a future supply one of no little interest. Its importance, however, results not so much from the quantity employed in construction, great as that certainly is, as from the rapid decay of the sills of railroads and the consequent necessity of frequent renewal. A resort to the harder kinds of wood in place of the softer was an obvious and early expedient; but little is thereby gained, for the former also harbor within themselves the germ of destruction, and under the influences of the atmosphere pass speedily through the stages of decomposition.

Of course this process is more rapid in certain kinds of wood, and is the effect of a greater proportion of cellular tissue containing nitrogen. It is to this that wood, exposed alternately to moisture and dryness, owes its decay, because the proteine substances, as the nitrogenous combinations are called, pass into fermentation, develop carbonic acid, and thus produce the gradual decomposition of the mass, although the second chief constituent of wood, the so-called cellulose, is in itself unalterable, and resists all destructive influences. The more proteine, therefore, contained in the wood, the more easily and earlier does it undergo decomposition.

Railroad sills of oak were found to last longer than those of softer wood, yet even these, though chosen with care, sufficed but for some ten years' service, and then for the most part required to be renewed. But through the exclusive use of oak for this purpose, it was soon observed that the forests were becoming thinned beyond all hope of restoration, and that the price of this wood had advanced to a most inconvenient extent. These annually increasing disadvantages have had the effect of directing inquiry to the practicability of replacing, for many different purposes, the use of wood by that of other materials, chiefly stone and iron; but for the sills of railroads this substitution has not been found to answer. Here, therefore, it was necessary to think of other means for prolonging the duration of wood, or at least for communicating to the soft woods, of which our forests are chiefly composed, a degree of durability which should qualify them to supply the place of the harder kinds.

Plans for the conservation of wood are just as little as the wasting of the forests an incident of yesterday or to-day. As early as the reign of Charles II, of England, Lord Caernarvon had said, "Wood is an outgrowth of the earth which nature provided for the payment of our debts," and the first proposal for the preservation of wood by chemical means dates from that period. Of this the celebrated Dutch chemist, Glauber, was the author. Two other proposed methods date from the last century, and since the beginning of the present a great number have been brought forward. All those heretofore devised, and which have had in view chiefly the preservation of railroad sills, depend—

1. On the abstraction of water from the wood before using it;
2. On the elimination of the ingredients of the sap;
3. On the chemical alteration of those ingredients;
4. On the mineralization of the wood.

Of the various proposals for this purpose we can here notice only those which

have obtained a certain notoriety. Among these may be classed that of Kyan, who proposed to steep the wood in a solution of chloride of mercury, or to press the latter into it. So strongly had this method been recommended that the building of the Leipsic-Dresden railroad was deemed a suitable occasion for putting it to the proof. The superstructure of this road was formed, after the former American system, of long wooden sills, strengthened by cross-ties, with iron rails attached, and these sills it was proposed to protect from decay by treating them in Kyan's much-extolled manner, with an infusion of chloride of mercury. The experiment, however, yielded a most unfavorable result. In the first place, the cost exceeded all expectation, amounting in the case of hard wood to $1,500 per mile, and in that of soft to as much again. Moreover, the solution had penetrated the hard wood to the depth of but two or three lines, and hence the protection was highly problematical. It is true that by frequent treatment or by pressure a complete penetration of the wood might have been effected, but in that case the expense would have certainly countervailed every advantage. In England the same experiment has been tried with several railroads, and also with the pins used in the wooden pavements of London. The sills of the London and Birmingham railroad were entirely decayed in three years, while those of the Great Western road, after six years, were still fresh and sound. These different results are attributable to the different modes of impregnation and to the contents of the respective liquids. The pins of the London pavement were found, after forty-six months, to be totally decayed.

Dr. Boucherie, of Paris, has acquired much repute for his method of preserving wood by means of copper vitriol, an expedient to which he was determined by long and sedulous experiment. So favorable were the results that, in 1856, after seven years' experience, large contracts were made with him for the impregnation of the sills of railroads and posts of telegraphs. In consideration of important public services thus rendered, the jury of the great industrial exhibition of Paris, in 1855, on the concurrent recommendation of two sections, awarded to Dr. Boucherie a large medal, while the public authorities, on the same grounds, extended his patent five years beyond the limited time. The basis, as well as the scientific principle of his procedure, is supplied by the assumption of the circulation of the vegetable sap, the existence of the cellular tissue, and of tubes within the plank through which this circulation is conducted. The second postulate is the possibility of displacing the sap and substituting a fluid possessing preservative properties.

In 1838, Dr. Boucherie obtained a patent for a process which depended entirely on the circulation of the sap. Upon this first system, a tree with its full garniture of boughs and leaves was sawed off, and its lower end sunk perpendicularly in the fluid, which thus ascended with the sap to the top of the tree. This process, though satisfactory in a scientific point of view, was not adapted to practical use. It remained to discover some means of causing the conservative fluid to penetrate into the felled tree without recourse to the natural circulation of the sap. Repeated experiments showed that it was practicable, by a high pressure, to expel completely the watery particles which remain for some time in the cells of felled trees, and to replace them by some other fluid. The problem was thus narrowed to the determination of a suitable fluid, and the application of a cheap and practical method of expelling the sap and introducing its substitute.

After employing, experimentally, various antiseptic substances, Dr. Boucherie obtained the most satisfactory results with a solution of the sulphate of copper (copper vitriol) in water. This substance, when introduced, is destined to a two-fold purpose—to expel the sap, which is the cause of decomposition, and to fix itself in the wood.

A small portion of the sap adhering to the inner walls of the cells is required for the fixation of the sulphate of copper; a combination of the two forms a covering which withstands external action, whether in the air, the earth, or the

water. Of this fact ocular proof may be had, if, by means of strong hydraulic pressure, we drive the albuminous substances from a stick of wood and prepare it after the prescribed manner. The vitriolic solution which is received at the end of the wood, where it flows out, possesses the same properties as at its introduction; there has been, therefore, little or no intermediate action. For every sort of wood there is a certain degree of pressure under which the preparation yields the best results. Nor is the strength of the vitriolic solution of less consequence than the force of the pressure: too weak, the effects are correspondent, unless much time is allowed for the preparation; by too great concentration, on the other hand, we injure the absorbing vessels of the cellular tissue, and the preparation becomes difficult, if not impossible; the wood in this case may be said to be scorched and corroded by the acids. The proportion recommended is a solution of one pound of the sulphate of copper in one hundred pounds of water. The water to be used for this purpose must be pure and as free as possible from calcareous salts.

All kinds of wood are not suitable for this impregnation. Certain kinds have isolated parts, where the sap is arrested, and no passage is allowed for the solution. In the oak, for example, the sap only is penetrable, while the pith resists penetration. The beech even, which is highly adapted to impregnation, often shows, near the pith, a red portion, in which the sap becomes inspissated and allows no passage. The birch and yoke elm admit of easy and thorough preparation, provided the age of the former be not more than forty nor that of the latter more than one hundred years. The pine, the linden, the plane, the service tree, the elm, and the aspen are well adapted to this purpose. In all trees the sap is the part most susceptible of impregnation, and this part, which is usually considered as unserviceable in constructions, the process of Dr. Boucherie renders fit for employment. The same is the case with many kinds of wood which grow in wet grounds, whose affluence in albuminous substances would, without such preparation, subject them to rapid decay.

For the success of the proposed method, it is indispensable that the juices of the tree should possess their full degree of fluidity, so as to yield readily to the pressure by which the preservative liquid is introduced. From the 1st of September, in many countries, but in general from the 15th of that month onward, the vegetable activity diminishes, the leaf changes color and soon falls. At this period the sap becomes thinner, circulates with more facility, and yields so much the more readily to the antiseptic liquid. Trees felled in September, October, and November may await preparation for a longer time, in proportion as they were later cut. The more advanced the season, the less is the tendency of the sap to coagulate and obstruct the vessels of the cellular tissue. In trees felled in October this condition scarcely supervenes before the end of November, while in those severed in January, February, and March, provided the boughs be left entire, the fluidity of the sap continues till the end of May. In general the sap of standing trees attains its highest degree of tenacity from the middle of April to the beginning of June; trees felled at this season, which is the most unfavorable, admit only of difficult and imperfect preparation. During the following months of June, July, and August the process should be applied within eight days from the felling of the tree, else the dryness, which promotes coagulation in the still otherwise tenacious sap, will tend to embarrass the operation and in some cases render it very imperfect. As a general rule it may be assumed that the most favorable epoch for the impregnation of wood is that in which the felling is generally considered as advantageous.

At whatever time the impregnation may be undertaken, it is always of great importance to select the soundest and straightest timber, and such generally as had not begun to decay and is free from clefts. The antiseptic liquid, on its introduction into the wood, will take the course where it meets with least obstruction, and if faults like those mentioned exist, will find through the yielding or divided parts a channel of escape.

II.

The measures above recommended must be observed if the subsequent steps are expected to result in success. We proceed now to describe the arrangements for conducting the process, which are very simple, and shall confine ourselves in the main to those intended for the preparation of railroad sills, merely noticing any differences which may exist in the case of materials intended for other purposes.

All the logs designed for sills should be cut into pieces whose length so far exceeds that of a double sill as to admit of the renewal, at the time of the operation, of the surfaces at the ends of the pieces where the sap soonest grows dense and obstructs the passage of the injected fluid. For this excess a length of 30′ is enough if the tree, especially in hot weather, be felled but few days before the preparation. To arrange the place of operation, we lay on a surface properly levelled four beams parallel to one another, with an inclination of $\frac{1}{100}$ in the direction of their length, which length should be such that at least twenty logs may be placed across the beams at an average distance of 2′ 6″ from one another.

Along the outer beams channelled logs or troughs should be laid for the purpose of receiving the liquid as it escapes from the ends of the sill-pieces; and the two inner beams must be placed at such a distance from the middle line of the whole construction as to leave between them sufficient room for a channel destined to receive a leaden pipe, which connects with the vessel containing the vitriolic solution. This pipe is furnished with copper taps at distances of 2′ 6″, corresponding with the middle of the sills which are to be impregnated. The fluid, which passes through the sills and falls into the channels provided for that purpose, is conveyed by them into a receptacle below the level of the general stage of operations, whence it may be drawn by a pump, and, if needed for further use, filtered and restored to its original strength.

The impregnating fluid is contained in three vessels, which are stationed on a platform, at least 25′ high in the middle of the works, and which are furnished with faucets, issuing a little above the bottom of the vessels, in order that impurities may have room to settle below the vent. Each of these faucets communicates by means of an India-rubber tube with the leaden pipe, which terminates at the sides of the vessels in three branches. Near at hand is a pump to supply the water required for the solution. Of the three vessels, one is designed to feed the leaden distributing pipe; the second receives the water raised by the pump, or the fluid that has been already once used; in the third, the prepared fluid is allowed to rest, that impurities may be deposited. This last vessel is connected with the distributing pipe as soon as the first is emptied.

The logs to be impregnated are laid upon the beams and wedged, so that their ends shall be perpendicular over the channelled logs or troughs, and their direction at right angles with the beams. Whatever may have been the lapse of time since the felling of the trees, the end surfaces of the logs should be renewed, that the injected fluid may more readily pass through, besides that the requisite length may thus be given to the material. Everything being thus arranged, an incision is made with a saw in the middle of each log to the depth of $\frac{2}{10}$ of the vertical section in soft, and still deeper in hard woods. By means of a jack-screw the middle of each log is then slightly raised, whereby the incision will be opened, and not far from this incision, in each half of the log, a hole is bored obliquely from the external surface of the log through the face of the incision, which must be carefully freed from any chips or saw-dust. In the incision we now lay a ring of cord or rope, the outer circumference of which must exactly correspond with that of the log; but, while thus preparing to close the opening, care should be taken that the ring do not descend too deep into the

wood and obstruct unnecessarily some of the tubes destined to convey the antiseptic fluid.

The screw by which the middle of the log was raised being now withdrawn, the log of course sinks, the two side walls of the incisiou approach, pressing together the included cordage, the circuit of the opening is completely closed, and thus an artificial reservoir is formed in the midst of the piece which is to be impregnated.

Into the hole, bored as above directed, a tube of hard wood is driven, and is fastened to one of India-rubber which has been previously adjusted to the copper taps of the leaden pipe, thus establishing a communication between the small reservoir in the middle of the log and the distributing apparatus. During the preliminary steps, the India-rubber tube is closely compressed by means of a hand-screw, but when, at the commencement of the process, this is removed, the impregnating fluid flows into the reservoir prepared for it in the log, and drives the sap before it through the pressure exerted by the fluid in the vessel which feeds the supply tube. Under favorable circumstances this effect is instantaneously manifested by an exudation at the end of the log, which presently changes into drops, and falls into the channel provided for it. To remove any included air which might interfere with the process, a small hole should be made with a copper pin through the cord enveloping the incision, which must be closed with the stroke of a hammer as soon as the fluid begins to issue. It will also greatly promote success, if in the course of the operation the incision be occasionally well cleansed, and again closed with the same care as at first.

The sap, which at first issues pure, becomes more and more mixed with the vitriolic solution as the process approaches its termination. When this mixture shows $\frac{2}{3}°$ (its normal strength being 1°) the penetration of the wood may be regarded as complete, and for pieces of the length of a railroad sill, the time in which this result is reached may vary from 48 to 100 hours; all pieces which, after the lapse of the latter period, do not exhibit in the centre of the end-surface a readily distinguishable impregnation, must be turned, and the operation conducted in the opposite direction.

For the preparation of longer pieces, such as telegraph poles, building materials, &c., in which the difficulties are greater, it will be useful to observe the following precautions: 1. To place the vessel which contains the solution higher, in order to increase the pressure. 2. To cleanse the imbibing surfaces oftener, with a view to remove impurities which may gather upon them. 3. To elutriate the fluid more frequently.

The arrangements for operating are like those above described, with the exception that here but two beams, laid parallel to one another at a distance corresponding to the length of the pieces, and with an inclination of 1 to 8, are required; the introduction of the solution will take place only at that end which shall give it the same direction with the natural sap; the artificial reservior ,constructed and closed as before described, will be near the but-end of the log, and the surface of the incision next to this extremity should be covered with a sheet of copper to prevent the penetration and escape of the impregnating fluid through the shorter section,

The acceleration of the process depends on the kind of wood, the season at which it is cut, and the effective pressure employed. Apart from these, the rapidity of impregnation may be assumed as proportional directly to the pressure and inversely to the diameter and the square of the length. Moist winds and snow hasten the process; dry winds and great aridity retard it; frost altogether arrests it.

This mode of preparation has been tried and approved by a number of the railroad and telegraph administrations of France. In 1856 more than 400,000 cross-ties thus prepared had been laid on the North road, 8,000 of the number having been deposited as early as 1846. In the former year these latter were

found to be as sound as they were the day they were laid, and this remarkable preservation, which they still manifest, leaves no room to conjecture the possible extent of their duration.

Expensive apparatus has been sometimes employed for the purpose in question; as, for instance, an air-pump, operated by a steam-engine, to exhaust the air from the wood, and thus facilitate the penetration of the metallic solution. Powerful hydraulic presses have also been used to promote this result. Such costly contrivances, however, may be wholly dispensed with. Büttner and Wöhring, of Dresden, among others, have proposed a process which is at once practical and cheap, and which quickly attains the desired end; their method has been consequently introduced in the case of several Saxon, Austrian, and other roads.

This method consists, as regards its chief feature, in the exhaustion of the air from the vessels of the wood—a condition indispensable to a rapid and thorough impregnation, not by mechanical forces, but exclusively by those of temperature. The whole operation is, in fact, conducted on this principle, the wooden sills being boiled for the space of an hour in a solution of metallic salts, and then left to cool undisturbed in the same until the temperature has sunk to 40° R. The physico-chemical process is here as follows: Through the heating of the wood to more than 100°, not only are the included gases but also the extractive substances expelled, the escape of the former being made manifest, throughout the operation, by the rising of large air bubbles, and the separation of the latter by a viscous substance floating on the surface of the solution and indicating even by its scent its vegetable origin. The wood, as it cools, being surrounded by the solution, rapidly absorbs it to supply the vacuity occasioned by the expulsion of air—an effect which is aided by the pressure of the atmosphere on the liquid surface exposed to it.

That the hot way for the impregnation of wood is decidedly preferable to any cold method of preservation would seem to result from the law that all organic chemical combinations are more certainly obtained in that way; besides that the contingency of protracted rains, which, in the cold process, sometimes wash away the metallic salts, is thereby avoided. At the same time, through the heat and vapor pervading the wood, a coagulation of the albumen may be occasioned, which probably, even without the intervention of the metallic salt, would of itself impart a preservative quality to the wood; for, as has been already said, the decomposition is to be solely ascribed to the ingredients of the vegetable sap, while the vegetable fibre in its simple state not only withstands the influence of the weather, but remains impassive under the sharpest reagents.

After persistent boiling for an hour and a half the heat will be found to have thoroughly penetrated the sill, and the highest rarefaction of the included air to have been attained; consequently the capacity of absorption will have also reached its highest point, which is estimated at 1¼ cubic foot = 62 pounds of the solution for a piece of wood of ⅞ cubic foot contents. It has been determined by many experiments that this is to be regarded as the maximum of absorption, which will not be increased even if the boiling be continued for several hours. As a cubic foot, = 50 pounds of the solution, is sufficient, therefore, for the preservation of a sill of pine wood, and its absorption is effected in one hour's boiling and from six to seven hours' cooling, it is apparent that the same apparatus may be twice used within twenty-four hours for the proposed operation.

The apparatus in use on the government railroad of Saxony consists, in the main, of a boiler of 10 horse-power, exerting a tension of two atmospheres, with a provision of four pine-wood receptacles, each 11½ feet high and 8 wide, for every boiler of the above description. The steam is conducted through an inch-wide tube from the boiler to the bottom of the receptacle, and traverses the latter through a tube of like width provided with small holes. The pieces

of wood to be impregnated are placed perpendicularly in the receptacle, with the larger end downwards, that the solution may ascend in the direction followed by the natural sap; a cover furnished with some openings, and well secured, is then applied, and first the solution and afterwards the steam admitted; the heating of the solution will thus be effected within two hours. It is to be observed that, as the volume of the solution is increased about one-fifth by the condensation of the admitted vapor, a proportional quantity of the metallic salt should be added to each receptacle to restore the reduced strength. Each of the receptacles is calculated to receive 40 railroad sills, of which 160 may therefore be prepared by such an apparatus as that above described within twenty-four hours.

III.

The chemical processes which take place in the impregnation of wood with the copper vitriol have been explained by König, of Dresden, through experiments made chiefly with pine wood. As regards the question whether the wood forms a chemical combination with the vitriol or one of its ingredients, he found that the oxide of copper, as well as the sulphate, is taken up by the wood, and that after washing the wood with water a saline base remains behind. If wood thus impregnated be closely observed, it is seen, from the green tincture of particular spots, that the metallic salt is deposited between the yearly rings of the wood in the less solid parts, and hence in those chiefly filled with sap. It has been further observed that wood abounding in resin takes up much more of the copper-salt than that which is deficient in it—oak wood, for instance, being scarcely stained by the solution. The woody fibre would seem, therefore, to have little or nothing to do with fixing the saline principle; it has been shown, indeed, that pure fibre—chemically prepared cotton, for example—does not combine with it in the slightest degree, but yields it up entirely through repeated washings. If, by treatment with alcohol, we obtain wood wholly free from resinous constituents and attempt to impregnate it, no color is communicated as in the case of resinous woods, and, by slight washing, the salt is removed. By evaporation of the alcoholic solution we obtain, under the form of a resinate, a greenish residuum containing resin and oxide of copper. It results from these interesting observations that the elements of the copper-vitriol are fixed in the wood through the medium of its resin.

If, with a view to a satisfactory determination of the question whether other ingredients of the wood may not co-operate in the fixation of the metallic salt, we examine the same wood before and after impregnation, it will be found that the impregnated wood contains less nitrogen, and that it is even possible, through continued treatment of the wood with the vitriolic solution, wholly to extract its nitrogenous constituents; these will be discovered in the solution. In this we find an explanation of the fact that impregnated wood resists decay longer than wood not thus prepared.

The preservation of wood by means of copper-vitriol depends, under all circumstances, upon the condition that the compound resulting from the union of the copper and resin should more or less completely fill the pores of the wood and invest the woody fibre, thus preventing the access of oxygen, and at the same time repelling the attacks of insects. These facts agree with the results realized in practice. It has been found that soft wood of loose structure lasts after impregnation longer than more solid wood, in conformity with the before-cited experiments, which show that the nitrogenous constituents are more readily discharged by the copper-vitriol from soft than from hard and heavy wood.

The experiments of König furnish, however, the mode in which the vitriolic impregnation may be most advantageously effected. With thin wood it is

sufficient, in order to extract the albuminous substances, to let it lie for some time, frequently moving the pieces, in a vitriolic solution of 1 to 2 per cent. Thicker wood must be treated with the heated solution in wooden or stone vessels, (since metal ones would be attacked by the metallic salt,) or be impregnated in the manner precribed by Boucherie. König thinks that when sometimes the experiment does not lead to the desired result, the failure is attributable to the mere steeping of the wood without allowing time for lixiviation, and the consequent discharge of unfavorable elements, which is the indispensable condition of success.

The preparation with copper-vitriol has been attended with satisfactory results in the case of several German railroads. In May, 1849, a commission of Prussian engineers examined the pine wood cross-ties which had been laid on the Berlin and Stettin road in 1841 and 1842. Here the impregnated and unimpregnated pieces lay close together. The latter were in general wholly decayed, while the former were in good preservation and still gave promise of long duration.

In England the chloride of zinc, which is much cheaper, has been proposed, and with highly favorable indications as regards the result. This process has been tried on the Hanoverian railroads, and it was found that sills which had lain for six years in the ground were still fresh and sound. Upon examination by Wöhler, it was stated that the chloride of zinc had penetrated, as well in oak as beech wood, deep into the material. From external indications this would not appear to be the case, yet here deception should be guarded against. In the oak wood chiefly a dark tint had spread to the depth of 1 inch to 1½, and it was thence concluded that the chloride of zinc had penetrated thus far; but this proceeded probably from a dark-colored deposit produced by the action of the tannin of the wood on the sides of the iron vessel. The mineral imparts, in general, no color to the wood, and chemical analysis remains the only means of determining its presence.

The greatest quantity of zinc was found in the beech wood, and in this respect no difference appeared in that which had and that which had not been steamed. With the oak it was otherwise, that which had not been steamed showing a much smaller proportion of the metal. Still poorer in zinc was the beech steeped in zinc-vitriol, and poorest the unsteamed oak treated in the same way. In the latter, therefore, steaming would seem indispensable, for only by a thorough penetration of the metallic solution can decay be permanently averted.

Recently a solution of the oxide of zinc in wood-vinegar has been proposed, and more lately still the chloride of manganese, which is produced in great quantity in the manufacture of chloride of lime, and as an incidental product is of little exchangeable value. The free acid is here saturated with lime or with oxide of zinc.

In North America, wood, especially that intended for ship-building, is salted, as with us flesh and vegetables are cured for longer preservation. This method can scarcely be recommended in our own practice, since, however calculated to prevent the so-called *rot*, the prices which we pay under a monopoly place the article beyond our reach, considering the quantity necessary to be used. It takes, for instance, for a brig of 6,000 cwt. burden, not less than 1,600 cwt. of common salt, and that is with us quite a capital. The salt might be replaced, indeed, by the mother-water of the salt-works, since great efficacy is attributed to the chloride of magnesium contained therein, which, in a chemical point of view, is very similar to the chloride of zinc.

It seems highly probable from the experiments of König that all these solutions of different kinds of salts, as far as they have succeeded in practice, act like the vitriol of copper upon the albuminous substances of the wood, and in like manner extract them therefrom.

H. Vohl, of Bonn, recommends the so-called kreosote (coal-tar oil) for the preservation of wood. This kreosote consists for the most part of an ethereal oil, with which small quantities of true kreosote and carbolic acid (phenylic acid) are mixed. Its practical examination is easy, requiring only that the oil should be mixed in a graduated cylinder with some ten per cent. of a strong alkaline lixivium, well shaken, and then left to settle. The liquid will separate into three distinct portions, the lower of which is purely an alkaline lye; the middle, which is brown, and of the consistency of sirup, contains the kreosote and carbolic acid; and the upper consists of the ethereal oil. As the volume of the substances employed is known, the quantity of kreosote and carbolic acid is easily determined. Since it is in these that the virtue of the impregnating oil resides, this criterion seems well adapted for determining the relative value of the latter. It has been stated that the coal-tar oil, received as well from England as from Belgium and France, contains a maximum of from eight to ten per cent. of kreosote and carbolic acid, whereas the preparation obtained from the photogenic manufacture is much richer in these constituents.

The presence of much ethereal oil in the fluid obstructs the absorption of the latter by the wood, and an eligible method for the preparation is to treat the kreosote with an alkaline lye, until, without being decomposed, any desirable quantity of water may be mixed with it; a certain proportion of the oil is separated, which is to be decanted from the mixture. The alkaline solution of kreosote, which, after the dilution, has a specific weight of 1.05 in relation to water, is applied by spreading it on the wood. When this application is absorbed, which soon takes place, the operation is repeated until the wood is sufficiently impregnated. Were the wood thus prepared exposed to the weather, a great part of the kreosote would be washed away; hence Vohl employs, for the fixation of the kreosote, a weak solution of the sulphate of iron, (iron-vitriol.) The sulphate of the vitriol neutralizes the alkaline menstruum of the kreosote, and this, now become free, attaches itself to the substance of the woody fibre. The precipitated oxide of iron, which entered together with the kreosote, is converted gradually into a hydrate of iron, at the expense of the atmospheric oxygen contained in the wood. The sulphate of soda (glauber salts) formed therewith is removed by degrees through the atmospheric moisture. Wood prepared in this manner, though exposed to every atmospheric alternation, exhibited, at the end of eight years, no trace of deterioration from decay or fungous formations.

Kreosote has been found of great advantage in the preservation of the rigging and sails of ships, not only supplying the place of tar, but excelling it in its beneficial effects. The efficacy of this operation rests on the facility with which kreosote combines with organic substances treated with lime, such as skins, leather, &c.; and in view of this the sails and ropes are first passed through a weak solution of lime and then through a strong tan-bath. The lime is precipitated through the operation of the tannic acid on the vegetable fibre, which thus impregnated readily absorbs the kreosote. Vohl observed no rottenness in sails thus prepared, after six years' exposure to all kinds of weather. In order to promote the duration of timber used in the construction of bridges, it has been proposed to protect those parts exposed to moisture and the atmosphere with roofing-felt.

Rottier, professor of chemistry in the University of Ghent, has recently made many experiments, with a view to discover which it is, among the various constituents of the coal-oil tar, that operates most efficaciously for the protection of wood from decay. His examination extended to the light or ethereal oil, the carbolic or phenylic acid, the aniline, the naphthaline, the insoluble residuum of the distillation, and the green, fluorescent oil which, redistilled at 275° to 320°, yields pyrene and parnaphthaline.

Of these elements, the light oil and the aniline evinced little or no efficacy. Wood saturated with the first lasted no longer than the same kind without it,

and the protraction of decay by the aniline might be expressed as being equal only to 6.66 per cent. Phenylic acid is known to be efficacious in the preservation of animal substances, and as the heavy tar oil contains it, the virtue of the latter was supposed to consist in the amount which it held of the former. But Rottier's experiments do not confirm this conjecture. Coal tar deprived of its phenylic acid proved as efficacious as that of commerce, which contains a large quantity. Napthaline has proved very effectual in protecting collectious pertaining to natural history from insects; but the presumption arising from this fact is not borne out by the experiments of Rottier as regards the preservation of wood. It is otherwise, however, with the heavy green oil; this evinced uncommon efficacy. It remained, therefore, to determine upon which of its constituents the virtue depends. Pyrene and paranaphthaline, on direct experiment, yielded no favorable results; whence it is to be inferred that it is the green oil itself which operates to the protection of wood from decay. It would seem also, from the experiments, that the higher the temperature at which the coal tar is distilled so much the more operative is it, probably from containing a greater quantity of the oil.

The fact should, however, not be overlooked that the experiments just mentioned have been conducted on a small scale, and the results have not remained uncontroverted. More decisive certainly are the experiments made with such materials as the sills of railroads, as well on account of the size of the materials submitted to trial as the parallelism of the circumstances under which the process is applied. Thus much at least we have already learned, that the preservation of wood, even to the extent which is now within our reach, is a subject which may well excite attention. If we had attained no other result but that of being able to impart to soft wood the durability of oak, and hence to substitute the former for the latter, this of itself would be of great importance, and be attended with many advantages to the various branches of industry in which the use of wood is indispensable.

CAOUTCHOUC AND GUTTA-PERCHA.

TRANSLATED FROM THE "AUS DER NATUR."

Through the discovery of America and the sea route to the East Indies, those prophetic words of Seneca were finally realized after so many centuries:

> "Venient annis
> Sæcula seris, quibus oceanus
> Vincula verum laxet et ingens
> Pateat tellus, Typhisque novos
> Delegat orbes, nec sit terris
> Ultima Thule."

Not precisely these, but similar dark legends, traditions of a remote age, in faint though recognizable lines, showed the route which Columbus and Vasco de Gama were to pursue. It was the fortitude with which these heroes braved the terrors of the ocean that gave them the victory. The one unveiled a new world, and the other brought India near to us, a country the charming aspect of which had, from the most remote time, kindled the enthusiasm or desires of mankind. These deeds soon produced their fruits; the unexpectedly expanded view opened a new era, and the inexhaustible resources which became accessible brought about a transformation of society.

The access to the tropical regions, over which nature has so lavishly strewn its rich treasures, became more and more easy; more and more of those precious gifts which the incessantly active though always still life of the vegetable realm works out there for man, the lord of creation, came to light and took rank among the necessities of civilized nations. Centuries have passed and the treasure is still inexhaustible—nay, still partly undisclosed. The London Exhibition has taught us this, its East India division displaying numerous natural products which we had not even known by name.

The abundance of light, heat, and moisture within the tropics creates there a vegetation of the luxuriance and splendor of which we of the cold north can hardly form an idea. The great fertility of the soil allows so many trees to grow up near each other that their branches find no room to spread. Thus every stem strives to overtop the other, pushing up towards the light, and far from the ground displaying its crown. Everything is so dense there that none can advance a step without opening a path with a chopping-knife. The ground itself is not large enough to bear all the plants shooting up in such rank exuberance; they themselves form a new soil for others, a soil which thousands of parasites contest with each other. All the fairy splendor spoken of in the ancient legend of the suspended gardens of Semiramis is here not only realized but surpassed. Here every tree is a true flower garden, rich in its variety of tints and forms. Raised high into the air on a single stem, these floating gardens look down from their giddy attitude upon the wanderer in charming gracefulness. With the manifold plants and blossoms that seem to shoot from the boughs of some trees, or to root themselves on them, strangely contrast those mosses which hang down from the branches of others like immense periwigs or horse-tails, or which, resembling beards, make the giants of the forest appear like gray veterans, whose heads the lapse of centuries has been insufficient to bend. But there is no path leading to the splendor of those luminous heights;

the traveller must content himself with a view from great distance, for more than one Cerberus guard the treasures. The external aspect, combined with the astonishing fertility and the superabundance of products of every description, suggests the idea that the garden of Eden, the paradise from which man had been expelled, has there again come to light, for the curse under which mankind groans—"In the sweat of thy brow shalt thou eat thy bread"—seems there to be powerless. But a shade is inseparable from light; there are hosts of terrors connected with those paradisiacal regions, lavishly scattered by nature in order to prevent man from easily enjoying his life amid all that magnificence. One example out of a thousand will make this clear.

Of the various branches of natural history, botany alone is regularly taught in our schools, which at least acquaints the youthful student with those offsprings of our flora which he meets in his rural excursions. Many of our readers will remember the surprise they used to feel at seeing a thick, milklike juice profusely flowing from some of the plants which they plucked. That juice was in some cases white like milk, in some colorless and dark, in other but rarer cases it was colored. Thus celandine, which we generally see growing around hedges and on heaps of rubbish, is all filled with a yellow juice, while the juice of some varieties of wolf's-milk is rose-colored. To the same class of plants belong, among the natives of our soil, the various salad plants, the poppy, the dandelion, &c.

The nearer we come to the equator the larger becomes the number of plants bearing a milky juice, and the greater the diversity of the qualities of these various juices. Just as the plants themselves mostly belong to the three great families of the euphorbiaceæ, apocyneæ, and urticeæ, in the same way we can divide the various milk juices, in general, into three classes. The first is the nearest in resemblance to animal milk; its taste is sweet, refreshing, and cooling, for which reason it is variously used by the inhabitants of those regions as an excellent means of refreshment. But to the plants themselves these juices are no aliment, as has been erroneously believed; they are in this respect by no means to be compared to the milk of the animals. The second class has become the most important to man. The fatty globules of animal milk are here replaced by a peculiar substance, which, like milk, is prevented from coagulating by an albuminous matter. Caoutchouc is here formed in the same way as cream out of milk at rest, and both possess that peculiar property that, when coagulation has taken place, a separation of the single globules can no more be brought about. The third class, finally, produces the most terrible poisons, which, in the hands of the aborigines of America, Asia, and Africa, become the most dangerous weapon against rapacious animals and against men frequently more rapacious.

Every part of the globe has its peculiar plants, which yield the chief component parts for these arrow poisons. They are mostly little known to us, the savages guarding their treasures with watchful jealousy. The preparation of arrow poison is a secret of the priests and sorcerers; it is accompanied, as is also the gathering of the milky juices for that purpose, with the performance of sundry superstitious ceremonies. He who is discovered selling the poison to Europeans is put to death; the purchaser shares the same fate. If the wound is only so deep that the poisoned point of the arrow penetrates to the blood, a violent convulsion of the limbs takes place almost instantaneously, which, in a few minutes, is followed by death, foam covering the lips of the victim, and very soon after by the decomposition of the body. The wounded man is irretrievably lost, for no European knows an antidote; the speedy cutting out of the wound and of its surrounding is said to be the only possible means of salvation. At least the natives make use of this means in order to save the flesh of the animals killed by them with poisoned arrows. Such flesh is entirely innoxious, in spite of the immediate effect of the poison, and is daily eaten in great quantities by

the savages. It is equally remarkable that many of the plants containing poisonous juices yield some of the most important means of subsistence in those regions. We mention here only the arrow-root, which, in tropical countries, is a substitute for potatoes; the yam-root, which has of late frequently been proposed as a substitute for the diseased potato in our own countries; and chiefly the manioc, (*zatropha manihot*,) which is to the natives of South America —colored, as well as white—what rice and the cereals are to the inhabitants of the Old World. Nay, our own potato offers an example of the same kind.

The plants which yield caoutchouc, now become an important commercial article, belong to all the three families above enumerated. The real caoutchouc tree, from which elastic gum was first extracted, is designated by the scientific name of *siphonia elastica*, and belongs to the euphorbiaceæ; it yields the greatest quantity, but many other trees of the same family yield smaller quantities. The best caoutchouc is derived from a plant of the family of the apocyneæ, called *cynanchum*. Further are to be mentioned here, *urceola elastica, Roxb.*, a plant of Sumatra; *vahea gummifera Poiset*, of Madagascar; *collophora utilis Mart.*, and *hanconia speciosa Mart.*, of Brazil; *willughbeia edulis*, of India, &c. Among the urticeæ the various fig trees deserve particular mention, (*ficus religiosa, indica, benjaminea, toxicarias, F. elastica, Roxb.*,) but besides them several other plants.

Tropical America and the East Indies are the great sources of supply. In the former it is chiefly the euphorbiaceæ, in the latter the fig trees, that yield caoutchouc for trade; while the plants of the family of the apocyneæ are rather common to both. As soon as the Caucasian race will grant the unhappy inhabitants of Africa the right of being men, rich sources will also be disclosed in this part of the world.

The genuine caoutchouc tree was first described by Aublet, under the name of *hevea guianensis;* but its blossom and fruit parts were not well known to him, they being first made known at a later period, by Richard, whose merits in making us acquainted with this useful plant were, however, subsequently passed over in silence by his own son. Wildenow subsequently referred the species to the genus *siphonia*. This tree grows sixty feet high, and about three feet thick; its wood is white, and its bark, especially on its very widespread branches, thin, grayish brown, and smooth. The Indians make long and deep incisions, reaching the inner wood, all around the tree, from which, the wound being kept open by a small wooden wedge, the milky juice flows spontaneously and profusely. To promote its drying, they make it flow in thin layers over moulds of unburned clay, mostly of the form of round and short-necked bottles of various sizes. The coating is repeated until the required thickness is obtained; the drying process is facilitated by fire, the smoke of which gives a black color to the gum; the moulds are then crushed within and removed in pieces. Formerly elastic gum used to come to us in strange shapes of birds, quadrupeds, &c.; now we receive it mostly in large plates, or blocks, or also in a fluid state, in hermetically-closed jars.

The collecting of the milky juice is done by the Indians with little care; the gum, therefore, contains many heterogeneous substances, which are an impediment in its elaboration. In general, the price of the article varies greatly in the regions that produce it, being determined by the quality of the merchandise, the size of the pieces, and the quantity brought to market. The commission sent by several German princes to examine the region of the Mosquito coast bought fifteen pounds of caoutchouc for five pence, (English.) Other natural products, like sarsaparilla, the collecting of which requires less labor, offer more gain, and thus the Indians, whose wants are easily satisfied, attach little value to elastic gum. But for this circumstance the exports from America would be considerably larger; a single man can collect sixteen pounds a day; however, more than

three or four pounds is rarely gathered. The inner bark of the tree is used by the Indians for the preparation of articles of dress.

The genuine caoutchouc tree is found everywhere in tropical America, from Mexico to Brazil. It chiefly abounds, however, in the extended plains south of the Orinoco, which are covered, so to say, by one primeval forest, and across which only the rivers, and especially the Amazon, can serve as roads, and in the numberless low islands enclosed by the exceedingly wide estuary of that gigantic stream. From this region caoutchouc is also exported in the form of shoes, manufactured by the Indians, who, for that purpose, make the milky juice flow slowly and repeatedly over the necessary moulds. Besides, considerable quantities are gathered around Quito, on the Mosquito coast, in Guiana, in the island of Mauritius, and in Brazil.

In the East Indies, the fig trees are predominant. Their numerous species, which chiefly form forests in low localities, invest the vegetation of the islands situated in the Indian Archipelago with a peculiar character, manifesting itself in their closed and sombre appearance, the density of the forests, and the moisture and dampness of the air. The stems of the trees rapidly develop themselves, and are remarkable for their bulky thickness, their irregular growth, and the wide spread of their intertwined branches. The wood, however, is soft and spongy, and a multitude of parasites and creeping plants spread a living cover over the bark of the stems growing out of the mouldering ground. Numerous hosts of apes leap to and fro, screaming and howling, over the high branches, and the thickets are all enlivened by the varied carols of the birds.

Already, when approaching the Straits of Sunda, the traveller finds a full compensation for the weariness of his long voyage, and the view upon the coast, teaming with vegetation, surprises him the more pleasantly as he still remembers the sparsely-covered heights of the Canary and Cape Verde islands and the bald summits of the African table mountains. The nearer he approaches, the livelier becomes his desire to enter the scene which so charmingly opens to his eyes. While Borneo is covered with forests displaying in the highest degree the character of equatorial exuberance, and Sumatra presents the aspect of a perfect tropical wilderness, Java, the finest of the Sunda isles, deserves the prize of beauty. Here the vegetable kingdom can be seen in its perfectly pure form; here more plainly than anywhere else can we see what the undisturbed power of vegetable growth in tropical climates, aided by a combination of most favorable circumstances, is able to achieve. In no other part, probably, of the eastern hemisphere is such luxuriance of vegetation to be met with. The whole island is a hot-bed reposing over a hearth of subterranean fire, still active and everywhere manifesting its activity. Just at the foot of the volcano Merapi, rising to an altitude of 8,000 feet, vegetation appears most powerful. Hundreds of species of trees, among which there is hardly one falling short of a hundred feet, form the high arched primeval forest, towering over a rich, spongy soil, covered with an endless multitude of mushrooms. Among these trees the urticeæ, or fig tree, are, in general, the principal figures.

Forming a continuation of the volcanic chain of the Sunda isles, there is another range of volcanoes, which takes a northerly direction: it is that of the Moluccas and Philippines. Chiefly in the latter islands, one of which, Luzon, is covered with a dense range of volcanoes, the gorgeous magnificence of the equatorial zone is fully displayed. If we there ascend the mountains, we perceive in the forests the powerful fig trees, around which luxuriant parasites wind a dense trellis-work.

On the Indian main land, and chiefly in further India, under the perpendicular rays of the sun, nature displays its full strength in developing the vegetable world. And, again, it is the urticeæ, together with terebinths, magnolias, gum trees, with large resplendent leaves, hairy silver trees, palms, bamboos, and similar plants, that produce a theatre of vegetation entirely new to the European.

It is true we find in our hot-houses the representatives of the plants which yield us caoutchouc, but we see them there only in a more or less stinted condition. However carefully we may rear and guard them, the animating glow of their native sun cannot be replaced. As the European becomes another man, if he does not entirely degenerate, in a tropical climate, so the character of those plants becomes altered in our artificial hot-houses; they yield no caoutchouc, their milky juice containing only a substance which greatly resembles our mistletoe glue. And this proves that the burning sun of the tropics is a principal agent in forming caoutchouc.

It is not exactly known who first brought elastic gum into Europe. Generally, it is believed that it was the celebrated French savant, La Condamine, sent by the French Academy to South America to partake in the measuring of degrees of the globe. On his return, in 1736, he is said to have spread the first knowledge of it. Later, in 1751, he more fully communicated his observations on the subject to the Academy of Paris. At that time elastic gum was still regarded as a great curiosity, to be found only in museums. The Portuguese were the first to introduce it, the commercial houses of Lisbon selling it under the name of *bococho*. In the far east caoutchouc was discovered by a company of soldiers who were compelled to cut their way with the sword through a forest of Prince of Wales island. They were surprised to find their blades covered with a glutinous substance, which proved to be caoutchouc. To Dr. Roxburgh we owe the first botanical description of the first East India plant, (*Urceola elastica,*) from which caoutchouc was derived.

For many years it was turned to no other use but the effacing of lead pencil marks. By degrees, however, the most important property of the hardened plant juice, its uncommon elasticity, became better known and usefully employed. In 1790 elastic bands were already manufactured; the art of softening caoutchouc and forming it into water-tight textures had been learned. In 1791 Grassert made caoutchouc tubes by twisting fresh-cut pieces, in the form of a screw, around a thorn. In 1820 Stadeler extended caoutchouc into fine threads, which were spun and woven into elastic textile fabrics. Later still, Mackintosh brought to market those water-proof fabrics which bore his name, and which, in a short time, made the tour of the whole civilized world, but just as rapidly fell into disfavor, the tightly-fitting dresses made of them proving to be inconvenient. For, in the same way as they kept off the rain, they also prevented the passage of the exhalations of the body, so that he who wore them for some time became wet even without rain.

That people also knew how to make use of caoutchouc in a different way, appears from Seume's "Walk to Syracuse." "Fine water," says he at one place, "is one of my chief favorites, and wherever opportunity offered I approached and drank of it. You must know that I am not so Diogenes-like in the matter as to drink from the palm of my hand, but that I use on my pilgrimage a flask of gum, which is clean, keeps well, and can be made to assume any shape." And again, when speaking of the insecurity of the high roads: "There is little reconnoitring with me; my hammer and my gum flask will tempt few robbers." But it is undoubtedly the chemists that turned the elastic gum to best advantage. We can say that it became indispensable to them. The successes achieved by that science since the end of the last century are to be attributed, in part, to the use of the apparently unimportant tubes, a few inches long, which the chemist so easily manufactures out of that gum. With their aid he makes gas conductors air-tight, and prepares many a complicated apparatus. This importance of the elastic gum is owing to the property of adhesion to each other residing in its fresh-cut surfaces. The chemist simply places a small piece of a thin sheet of caoutchouc in such a manner over a glass tube of convenient diameter that the two edges flatly overlay each other, and then rapidly cuts them off with a sharp pair of scissors. If the cut edges do not

touch each other he presses them carefully together, and the tube is done. The value of those tubes is enhanced by other chemical properties of the substance, which is insensible to the influences of many acrid liquids and vapors. Chlorine gas, hydrochloric, and many other acids and caustic alkalies, do not affect it. Concentrated sulphuric acid causes a carbonization of its surface, but a further decomposition only at high temperature, sulphurous acid being evolved, and the gum assuming the softness of resin. Nitric acid makes it yellow, and, after some time, soft. In the fuming acid it dissolves, evolving carbonic oxyd. But a mixture of concentrated sulphuric acid and nitric acid acts most destructively.

The common solvents exercise no influence on caoutchouc. The solution of this substance was, therefore, a long time an enigma to chemists, even after Macquer's pretended discovery in 1768 of the key to it. It was Pelletier who first indicated the right way of doing it. Having been softened in hot water, caoutchouc is solved by ether freed from spirit of wine. Without this precaution the substance is only softened, as it is by petroleum or spirits of turpentine. At the same time the substance increases by swelling even to thirty times its volume. It is an easy matter to blow the softened flasks into a considerable size. Mitchell expanded a caoutchouc bladder of the size of a walnut to fifteen inches diameter. He prepared such balloons, some measuring six feet in diameter, which, being filled with hydrogen gas, serve as toy balloons for children. One of these balloons having slipped from his hand, came down to the ground only at a distance from the town of a hundred and thirty miles. But caoutchouc swelled in rock or turpentine oil can be so much extended by the application of heat and mechanical means, larger quantities of the liquid being gradually added, as to appear dissolved. The caoutchouc membrane, however, which is left after the evaporation of the solvent, has the inconvenient property of long remaining sticky. To remedy this inconvenience, Benzinger has by accident discovered an efficient means, not yet widely known, in the admixture of a very small quantity of a concentrated solution of sulphuret of potassa.

Better solvents for caoutchouc have lately been discovered. Such are chloroform, sulphide of carbon, and chiefly those carburetted fluids which are derived from the distillation of tar or of caoutchouc itself. In one factory at Greenwich, England, about eight hundred pounds of waste caoutchouc are daily subjected to dry distillation in iron vessels. When this operation is not carried too far there remains a greasy mass, which retains this property, and effectually withstands the influence of the air and water. For this reason it is used in England for the purpose of saturating cables, and thus rendering them more durable. A similar greasy mass is gained by melting caoutchouc at a temperature of 125°. It swells and burns with a bright whitish flame, so that in South America caoutchouc is used for purposes of illumination instead of candles and torches.

In the mineral kingdom, too, substances are found which in their external properties greatly resemble caoutchouc, but resist the power of solvents still more effectually than the vegetable material. A similar substance can be produced artificially by exposing thin layers of linseed oil to the air for six or seven months.

As early as a quarter of a century ago caoutchouc had become a branch of industry of some importance. But the English imports of the raw material show that it was still resting on an uncertain basis. While in 1829 about 100,000 pounds of elastic gum was imported, its consumption in the following year reached only half that amount. In 1833 a duty was paid upon no less than 178,675 pounds. The properties of the raw material itself greatly restricted its manufacture. The use of the articles manufactured was changeable and limited. By moisture and cold gum-elastic partly loses its elasticity, and becomes hard, but it is softened by heat and compression in the hand. Many an article that was very useful in summer had to be put aside in winter. The

removal of this inconvenience gave an expansion formerly unthought of to this branch of industry, which became manifest at the world exhibitions of London, New York, and Paris, in 1842. The importation of caoutchouc into England amounted to 750,000 pounds; and about the time of the London Exhibition one South American port alone exported yearly no less than 4,000 hundred-weight. By what means this rapid change was consummated may be described in another place; here we find it more convenient to introduce the lately discovered companion of caoutchouc. The manufacturing processes of both are almost identical, and are therefore to be treated in conjunction.

During one of his travels in the East Indies, Montgomery, surgeon of the Singapore East India Company, entered into conversation with a Malay laborer. While talking, he observed the handle of a hoe, and he heard with surprise that its substance, however hard it appeared to be, could be softened by immersion into hot water, and could thereupon assume and preserve any desired shape. The experiment being immediately made, the assertion of the Malay was fully confirmed. On further inquiry that excellent quality of the substance in question was found to have been long known among the nations of Java, where it was used for manufacturing canes and handles of whips, as well as of various other implements, and especially of knives and daggers. Montgomery was induced to send, in 1843, various specimens to London, and to call public attention to the manifold uses of which the thus examined substance was capable. His words were more duly noticed than those of D'Almerida, who about ten years before had sent a similar freight to the Asiatic Society in London. The Society for the Encouragement of Arts and Industry bestowed a gold medal on Montgomery. But gutta-percha was at the same time also discovered by Thomas Lobb, who, in 1842–'43, made a botanical journey through the East India islands.

The new, but now already so well-known substance, is also a dried milky juice, in many respects resembling caoutchouc, and, therefore, considerably used of late as a substitute for it. The Malay name, gutta-percha, is applied by the natives to an inferior sort, derived from a tree as yet unknown to us, probably a species of fig-tree, while our gutta-percha is called gutta-taban by the islanders.

The mother plant was for a time unknown until Oxley sent to Hooker, in England, some blossoming specimens from Singapore, the principal place of exportation. The parts of the plants were enclosed in a gutta-percha box, and reached thus well preserved the hands of the botanist. He recognized the plant to belong to the genus *isonandra*, lately introduced by Wright, and to the family of the *sapotaceæ*, and gave it the name of *isonandra gutta*. Here we must remark that gutta is not the latin word for "drop," but a Malay word designating "tree's sap."

The tree attains an altitude of forty, and according to some even of sixty or seventy feet. The stem is straight, and often from three to six feet in diameter; its blossoms, four in a bunch, are small and white; the fruit is sweet, and yields a fat useful in the preparation of some kinds of food. The wood is soft, fibrous, and spongy; it contains numerous oblong cavities filled with the milky juice, and forming broad streaks. Unfortunately the way of procuring gutta-percha is exceedingly crude. It is not done by making incisions in the trees, as is the case in gathering caoutchouc, but by felling the stem, some grown to an age of from fifty to a hundred years, peeling off the bark, and collecting the milky juice in a trough made of the stem of the *musa paradisiaca*, in cocoanut shells, &c. The juice, when spread out in thin layers, soon becomes solid in the open air; larger quantities are thickened by heat. The yield of a tree is said not to exceed thirty pounds.

Although the tree abounds over a vast extent of territory—in the island of Singapore, in the forests of Johore, at the extremity of the Malay peninsula,

in Borneo, Sumatra, and all the numerous islands of the straits of Singapore and the Indian Archipelago—the destruction of the stems, (270,000 in all,) caused by the rapidly increasing exportation—22,225 pounds in 1844, but already 25,533 cwt. in the following three years—gave rise to serious apprehensions. And in fact, the exportation itself seemed to decrease soon after, for, after reaching 11,114 cwt. in 1846, it amounted only to 8,091¾ cwt. in the following year and a half. It appears, however, to have since somewhat increased.

Be this as it may, warnings were sent over from England to treat the trees more savingly, and to make incisions in, instead of felling, them. Frequent complaints were heard that in India people believed the primeval forests to be inexhaustible. That they are not so, and what pernicious consequences follow their devastation is proved best by the example of the West India islands. At the time of their discovery, these were all covered with the finest forests; now fine forests are only to be found in the larger islands, which owe to them their abundance of water and fertility. The smaller islands, however, the forests of which have been recklessly destroyed, suffer from drought, and in part possess neither springs nor brooks. There is scarcity of fuel all over the West India islands; in Cuba the sugar pans are heated with orange-wood, the sugar-cane not being sufficient for the purpose. Though this example speaks loud enough, people in the East Indies have no ear for the warning, and seem bent on reproducing there the deplorable condition of the West India islands. The earnest entreaties from England remain unheeded, and the well-known botanist Seemann informs us that he could discover no gutta-percha tree even around Singapore. At some places the tree is grown by European settlers in gardens. The coasts of the Indian Archipelago, too, are already greatly denuded, while transportation from the interior is connected with great difficulties.

Gutta-percha is brought to market partly in a liquid state, partly in small slices, or kneaded into blocks and rolls, in the cuts of which the layers are distinctly visible of which the whole mass is composed. In this case it is solid and hard, but still easily receives impressions by the nail. The color is more or less reddish brown, owing to pieces of bark contained in the mass. This contains, besides other substances, parts of plants of various kinds, sawdust, earth, &c., admixed with dishonest intent, and sometimes amounting to a fourth of the whole, especially as the trade at the principal place of export is almost entirely in the hands of the crafty Chinese.

Gutta-percha, in itself, possesses little, if any odor; but it often smells strongly, as of rotten cheese, or of something sour, on account of admixed substances in a state of fermentation. At ordinary temperature, 0° 25′, it is hard, leathery, solid, and strong, so that, for many purposes, it is preferred to wood or horn; it is tough, very stiff, and little elastic, so as not to resume its original shape after much bending. At no temperature does it possess the elastic ductility of caoutchouc. It offers a considerable resistance; its solidity has been tried by various experiments. According to MacCayan a piece one-eighth of an inch thick is torn only by a pressure of 50 pounds. Payen put gradually increasing weights upon a very thin gutta-percha band 7½″ long, 1″ 4½‴ broad, and not fully $\frac{14}{1000}$‴ thick, until it tore; this took place under a pressure of more than 4½ pounds, the band, in the meanwhile, having expanded to 1′ 1½″, that is to say, almost to double its length. Fermantel has found that every square line of the diameter of a gutta-percha band could bear, before tearing, a pressure of 25 pounds. The line of division marking where the elasticity begins to be tasked, would, according to these experiments, fall on five pounds for every square line, or 720 pounds for a square inch. At a higher temperature, by which gutta-percha is softened, tearing ensues much sooner, and the latter, therefore, frequently proved a failure when services were demanded of it which did not agree with its nature.

The most remarkable peculiarity of this substance, which makes it appro-

priate for so many uses, is its relation to heat. Above 50° it becomes more flexible and somewhat elastic, but still maintains its hardness and remarkable power of resistance; on severe tension its contracts but little. At 60° to 70° it becomes soft and very plastic, and loses much of its toughness. In this condition several pieces of it can easily be kneaded into one whole. Mere immersion into hot water of these temperatures suffices to give the mass every shape desired, which it also preserves after cooling off, when it reassumes its former hardness at every temperature below +45°. It is, further, easily inflammable, and burns with a bright flame, and amid strong sparkling, dropping off a dark, glutinous residue. In regard to solvents, gutta-percha is like caoutchouc; its decomposition in increased heat and its composition are similar to those of that substance. Like it it consists of carbides of hydrogen. The little oxygen which has been found in analyzing it was probably received from the air during the purifying operation. The article of trade has frequently been subjected to chemical experiments, but we cannot say that a clear result has been elicited. We omit them, therefore, mentioning only one circumstance which has often caused confusion in practice.

The surface of a carefully cleaned plate is found to be, in parts, covered with a bluish bloom, which, however often wiped away, reproduces itself as long as the plate remains flexible. After years the whole surface appears to be faintly grayish blue, and under the microscope can be perceived an exceedingly thin layer of very fine, white, little dots. A higher temperature, to which the gutta-percha had been exposed, greatly promotes this alteration, and therefore the darker sorts suffer by it most. The physical property resulting from this change of the surface is noteworthy. Unchanged gutta-percha is a good insulator of electricity, and occupies so low a rank in the scale of electric affection by rubbing as to remain strongly negative when rubbed by almost any substance. Only gun cotton and electric-paper make it positively electric. The changed surface does not destroy the power of insulation, but the gutta-percha rises thereby high in the scale of electrical excitement, and when rubbed with almost any substance becomes positively electric in a high degree. The exceptions are only mica, diamonds, and furs. Hence we learn the remarkable fact that we possess in gutta-percha a plate, one plane of which—the blue—when lightly rubbed with the hand, with linen, glass, rock crystal, the barb of a feather, or flannel, develops intense positive, and the other—the brown—intense negative electricity when going through the same processes.

Oesdorf finds the cause of this change of surface, which resembles a similar appearance on the surface of ripe plums, in the attraction of water from the atmosphere. Gutta-percha, deprived by cautious melting of all water, of which it, as a rule, imbibes some 5–6 per cent. in the process of formation, and therefore assuming a dark brown color, is soon covered, especially on its cut planes, with this vapory coating; while this does not take place, or does only a great deal later, with the light-brown substance, which has not been rendered anhydrous to so extreme a degree, except when, and only where, it is traversed by dark veins. Still, it is a question whether the change of color is a consequence of a mere deprivation of water, or rather attributable to the influence of heat, that is, to a change of the substance itself.

Therefore, it is likely that the change of the gutta-percha is caused by a secretion of a component part of the mass by the influence of air and heat. The excessive positive irritability of the altered surface, which we find in no other vegetable substance, is an indication that the process is connected with the two kinds of resin which Payen has obtained from gutta-percha. A closer inquiry into the question, especially in the indicated direction, would be useful, for only thus could we discovar the causes of the unfortunate transformation of gutta-percha into a very brittle mass, a change noticeable chiefly in small articles manufactured out of that substance.

Caoutchouc and gutta-percha can be easily distinguished from each other. First, they are entirely different in their structure. When gutta-percha is rolled into thin sheets, or drawn into strips, it is like a fibrous substance, which is not the case with caoutchouc. A thin strip of gutta-percha can be considerably stretched in one direction, in that of the fibre, but it tears at every attempt to stretch it obliquely to that line, while caoutchouc is easily stretched in every direction. When thin sheets of both substances are examined in their relation to light by means of polarization caoutchouc shows little, if any, change of color, while gutta-percha offers fine appearances. The latter seems to be constructed of prisms of the most variegated color, which appear to be interlaced in each other.

In a chemical way, too, the two substances can be distinguished from each other by means of chloroform. Gutta-percha is dissolved in it boiling ; it yields no ether from the solution by distillation, but an alcohol in the form of a white, ductile, not sticky membrane, and such is its residue after the evaporation of the solvent. Caoutchouc, on the other hand, swells up in boiling chloroform, and only when the jelly has been divided by chemical means, perfect solution ensues in the further process of boiling. Alcohol acts here as a means of distillation; the caoutchouc is secreted as a coherent, not as a sticky mass. In this way the two substances can be detected even when mixed together.

Of the formation of these two vegetable substances we know nothing, and neither do we know more of the part they act in the organism of plants. In order to avoid the difficulty, they are designated as secreted matter.

When Montgomery sent to London the first specimens of gutta-percha he chiefly recommended it for the purpose of manufacturing surgical instruments, as those made of caoutchouc would soon become softened and glutinous. It was, however, soon found out that the excellent qualities of gutta-percha render it, much more than caoutchouc, appropriate for a thousand other uses; it especially promised to become a substitute for leather, showing none of the disadvantages presenting themselves at the application of caoutchouc for the same purpose. Besides, it is not subject to wear and tear; and when the shape into which this pliable mass is cast gets out of fashion, it has only to be put into hot water in order to be transformed, or used for something quite different. Although known only for a short period of time, it is already used for the manufacture of so many things, and the ways of manufacturing it are so manifold, that it will be difficult to present here a true picture of the whole.

The operations used in working these two substances are common, in part. The first is that of cleansing them from foreign admixtures, the black Java caoutchouc, particularly, containing a considerable multitude of small stones and vegetable particles. At the purchase of caoutchouc the quantity of water it contains is, above all, to be taken into account, that admitting of deceitful augmentations. For not only is thereby the real value diminished by a fourth, but it also loses considerably of its toughness and ductility, while its whiter color apparently indicates a better sort.

The cleaning operation is executed partly in a mechanical and partly in a chemical way. The mass passes between two turning cylinders, corrugated, overlying each other horizonally; this removes the stones and similar substances. In order to make this process complete the rolled sheets are washed in lye. A large number of these leaves are pressed together in a heated cylinder, which gives them great uniformity. When in large plates caoutchouc is put under a hydraulic press, where it is subjected for six or eight days to a severe pressure, at a temperature of from 45° to 50°. When very thin sheets are to be obtained, the heated mass passes through a rolling machinery, the hollow cylinders of which are heated to a temperature of 100°, by means of steam or of hot iron bars, and which are gradually placed tighter and tighter. The mass being very adhesive in heat, sheets of every length can be obtained

by fresh additions to it during the process of rolling. In order [illegible] be prevented from sticking to each other, they are, [illegible] placed into cold water or strewn over by talc powder. [illegible] textures or covered with them on one side, and then [illegible] caoutchouc and texture adhere to each other, thus forming a very [illegible] water proof material, which is not, however, free from defects. By pressing the edges together, while heated, the caoutchouc sheets become [illegible] so that in this way various utensils, bags, &c., of all dimensions [illegible] be manufactured of them. This property made [illegible] could soon do without tailors. According to him, instead of being sewn, the edges of the single pieces of a dress are to be besmeared with [illegible] and pressed together.

Plates of caoutchouc and gutta-percha are easily cut into threads and ribbons of any thickness by two spiral blades of a peculiar cutting machine. In order to facilitate the weaving of caoutchouc threads, they are made non-elastic by stretching and cooling. The finished texture being heated to a temperature of 45°, the caoutchouc regains its former elasticity. Both caoutchouc and gutta-percha can also be drawn into very fine uniform threads by means of a drawing iron, the material being swelled up in sulphuretted carbon, containing an admixture of alcohol. Thick threads can be stretched into [illegible] six times their length, and when heated to a temperature of 100° they preserve the length obtained by stretching. After cooling, the same process can again and again be repeated, until the desired thinness is obtained. After six repetitions, for instance, the length obtained is already like 16.625 to 1, representing the original length. Such thin threads of gutta-percha are particularly to be recommended as twine to artistic gardeners. Their pliability and strength are astonishing. Plaitings derived from them seem to be indestructible.

When caoutchouc is to be used as dough or cement, it is first made to swell in about double its weight of the solvent, and the jelly is then crushed in a machine by means of several cylinders. This mass serves either as glue, as, for instance, in the manufacture of cabinet ware and musical instruments, or to make certain materials water-proof, or to save them from moisture, as, for instance, the wainscot of buildings. After the example of England, German bookbinders, too, use caoutchouc, instead of animal glue, to great advantage. A caoutchouc cement, known under the name of mairne glue, has evinced some excellent qualities, especially in joints of wood and metals exposed to water, as well as for calking purposes. A solution of caoutchouc and rapeseed oil, which absorbs of the former only a one-hundredth part of its own weight, is used as grease for parts of machines exposed to excessive friction. When slaked lime is added to the softened mass which caoutchouc yields at a temperature of 210°, an excellent water-proof cement is obtained. Though tough, the mass remains kneadable for years; an admixture of vermillion is, therefore, necessary when drying up is desired.

A solution of caoutchouc and gutta-percha, has various useful applications, among others for coating various substances in order to make them water-proof or to save them from the influence of water and air, as, for instance, in the manufacture of leather, shoes, and boots, leather and hemp bags, cordage, &c. A solution of gutta-percha in chloroform is more serviceable in curing wounds than collodion, which it will undoubtedly replace also in photography.

Within this narrow sphere the caoutchouc industry but a few years ago moved. The discovery of the remarkable relation of this substance to sulphur finally removed all defects from the articles obtained by its manufacture. The English manufacturer, Hancock, early discovered that the hardened vegetable juice enters into a chemical combination with sulphur, which greatly alters the property of the former, and makes it entirely indifferent to the influences of the temperature. Under all circumstances the principal property, elasticity, re-

mains unchanged. Sulphurized, or, as it is called, vulcanized caoutchouc becomes neither soft nor glutinous in a tropical heat, nor hard and brittle in the cold of a northern winter; even a temperature of 100° and upward has no effect on it. Solvents, too, lose all their power of affecting it.

But before industry could draw considerable advantage from this circumstance, various experiments were required, for the discovery of the precise relation of sulphur to caoutchouc, and of the exact temperature to which the mixture was to be subjected. The elucidation of these difficult points we owe to the American Goodyear, through whom the caoutchouc industry has risen to a height never dreamed of before.

According to the earlier way of proceeding, the sheets of caoutchouc were laid in fluid sulphur, of which they absorbed from ten to fifteen per cent. within two or three hours. This causes no alteration in the properties of the organic substance, while at a temperature of from 135° to 160° such an alteration is brought about in a few minutes. A longer subjection to this temperature is injurious; the manufactured article becomes less pliable and elastic, and becomes hard and brittle, so that it tears off short even on a slight stretching. The same occurs when too much sulphur is absorbed, and this is under that method always the case, only from one to two pounds of sulphur chemically uniting with the organic substance. The rest remains lying between the pores, and is removed by mechanical means by alternate stretching and contraction, or rather by chemical substances acting as solvents, as alcoholic lye, ether, sulphide of carbon, oil of turpentine, or benzine. The latter process must always be applied when the vulcanized caoutchouc is brought in connexion with metals, which the residue of sulphur not chemically united would affect injuriously.

The difficulty of observing the right measure of sulphurization is proved by the manifold complaints about the inferiority of the manufactured articles in the market, which by no means possess the so much vaunted excellent qualities. It is easier to observe that precise limit under the methods of cold vulcanization recommended by Parkes and Gerard. The caoutchouc, or the article manufactured from it, is steeped in a mixture of chloride of sulphur and sulphide of carbon, or in sulphide of potassium. But in whatever way the sulphurization is effected, it does not take place uniformly over the whole of the mass. To judge by its reaction with solvents, there seem to be two different compounds, besides which there is also unaltered caoutchouc.

Although gutta-percha adapts itself to manufacturing processes more easily than caoutchouc, it yet gains by sulphurization, becoming thereby more elastic and insensible to changes of temperature as well as to solvents. Better still than pure gutta-percha is a mixture of both substances.

It can be said that sulphurization has given an endless variety to the use of those substances. Nothing could prove this more than a visit to the industrial exhibitions of London, Munich, and Paris. At the first named, extraordinary interest was excited by a collection of gutta-percha manufactures, prepared by natives of India, to the exhibitor of which (W. Kerr, of Singapore) the prize medal was awarded.

The chief representatives of this branch of industry are North America, England, and France, and the master in it is Goodyear, whom his invention promises to make immensely rich. Not only the manufacturers of his country are tributary to him, on account of his patent, but also numerous firms in France. For a time Goodyear was the lion of the whole Union. A countryman of his, however, (Mr. Day,) contested his right to the monopoly, and this gave rise to a gigantic lawsuit, which agitated the whole Union, the most celebrated lawyers, and among them the great statesman Daniel Webster, pleading on the one side or the other.

The manufacture of India-rubber goods was everywhere very limited before the invention of vulcanization; since 1844, however, this immensely

[illegible] invention has opened to the plant-juice almost all branches of human industry. The importation of that gum and the amount of goods manufactured from it are increasing with astonishing rapidity. Most of the factories are in the States of New York, New Jersey, Massachusetts, Rhode Island and Connecticut. Thousands of people find there employment and gain. The work is chiefly done by boys and girls, but adult men, and even [illegible], are variously employed. One of the most important articles is springs for railroad cars, the patented monopoly for which, in the Union, belongs to the "New England Car Spring Company," which yearly works up about 400,000 pounds of the raw material. For some other articles its consumption is equally immense. Thus several million pairs of India-rubber shoes are yearly manufactured in the United States, the "Hayward Company" producing daily several thousand pairs. Some of the finest shoes, such as would preserve their gloss after the longest sea-voyage, are manufactured by "Hartshorn & Co.," in Providence.

And yet the working up of this peculiar product of nature is still in its infancy; every day discloses new ways of using it. Already attempts are being made to coat the submarine telegraph wires with India-rubber, and this substance is also to be used for nautical charts, instead of paper. As it can be rolled into sheets of the greatest thinness, it seems to be destined to replace paper in various respects. Maps, globes, &c., are already prepared from it. An extraneous circumstance will promote this development. It is already regarded as a fact that the consumption of paper is now out of proportion to the production of the raw material necessary for it, to wit, rags. All efforts to check the increase of this disproportion, through the use of other raw materials, have as yet produced but an incomplete—by no means effective—result. This is particularly noticeable in the North American Union, which, as of so many other things, can boast of a grand journalistic and other literary productiveness, and vainly looks for raw materials in the European market for its immense paper consumption. The remedy will not be long sought for; the indications are already given. Bleached gutta-percha, especially, is better adapted for lithographic printing than the finest Chinese paper; it yields really admirable copies. By wetting it with a solution of gutta-percha in sulphuretted carbon, printing paper can most easily be transformed into writing paper.

A peculiar branch of this new industry is the now immense production of toys for children. However sad a part the German may play in his own house, and however grievous the offence this subjects him to on the part of proud nations, the honor of being the teacher of mankind remains to him intact. German scientific industry labors for the benefit of the whole universe. Thus Nuremburg can boast of having been for centuries the privileged teacher of the children of all nations and races; its toys contributing to develop the first ideas in the children's world. But this privilege is now contested by the industry of the United States. Instead of the harmless dogs and cats imported from Nuremberg, babies receive there as toys India-rubber eagles, horses, lions, and leopards, destined to rouse their power of observation. The importation of German toys has suffered by it, but the industrial products of Nuremberg, or what goes by that name, will not easily yield the ground. At the Paris exhibition we saw a host of those North American toys, but these were deficient in neatness and taste; they entirely lacked the gracefulness of the Nuremburg, and especially of the Wurtembergian manufactures, which at the Munich Exhibition so vividly brought back to our mind all the charms of our half-forgotten childhood. Already at the London Exhibition the new American articles of India-rubber manufacture became objects of general attention. Charles Goodyear, of New Haven, Connecticut, and Charles Mackintosh & Co., the two principal representatives of the caoutchouc industry in America and England, were the

only receivers of the great council medal, other prize medals being awarded to two American, three English, two French, and one German firms.

The American division of the Paris Exhibition owed its principal attraction to its numberless India-rubber articles. A new kind of vulcanized caoutchouc was chiefly noticed—another triumph of Goodyear's inventive genius. We refer to the so-called hard caoutchouc. When caoutchouc receives an admixture of about a fifth of its weight in sulphur, the mixture being heated to 150° and some asphalt being added, a mass is gained which equals marble in hardness, and is capable of an extremely beautiful polish. The manifold applications which this valuable invention has already found, allow us to realize the extraordinary extension of which that branch of industry is capable. Hard caoutchouc is a substitute, not only for ebony, horn, tortoise-shell, ivory, and whale-bone, but also for iron, which so easily rusts in damp air.

People gazed there with admiration at handles of knives, and rifle stocks, adorned with the finest and most artistic reliefs, at opera-glasses, and a thousand other optical instruments, or articles of cabinet ware, which were formerly manufactured of ebony or buffalo's horn. There were also exhibited richly gilt pieces of furniture, wrought entirely of this new material, as well as articles of vertu set with genuine pearls, and various utensils ornamented with Chinese paintings. We observed further musical instruments, such as violins, clarionets, and trumpets. Whenever we visited the exhibition we could not refrain from admiring the exercises executed on one of those trumpets, shortly before the close, which was indicated by the ringing of all the bells contained in the building. To make the contents of the whole collection more complete, we must add candelabras, an electric machine, very flexible whips and canes, surgical instruments of every kind, powder-horns, various seals, printing type, spools, shuttles, slates. Large sheets of hard caoutchouc, destined for the plating of ships, attracted particular attention. The low price, the slight weight, and the indestructibility of this new material will soon entirely supersede the now usual copper plating. At Havre and New York the new method of coating vessels has already been tried in the dock-yards of most prominent ship-builders. Ships have sailed from both ports for long voyages, and nobody doubts that on their return the theory will be confirmed by experience. At Plymouth, England, on the proposition of Mr. Forster, of the royal navy, the outsides of plank are coated with gutta-percha.

Neither was the art of printing forgotten in this rich collection A thick quarto volume contained the history of that branch of industry on which we here comment. The leaves challenged destruction by water, being made of vulcanized caoutchouc, as the elegant binding was of the kind designated as vulcanite. It is greatly to be regretted that this invention was made at so late a period; made earlier, it might have saved us many treasures of antiquity. The deluge itself would have been powerless to destroy such written monuments.

Of the articles of vulcanized caoutchouc contained in the American division we must chiefly mention, besides toys already alluded to, maps, a great variety of water-proof articles of dress, water-proof military tents, and very carefully worked pontoons.

Young as this branch of industry is, it has already achieved wonders. Not content with alleviating the sufferings of man, gladdening the heart of children, and saving from floods the manuscripts of authors in the archives of nations, it continually seeks new paths for its progress. Side by side with the neatest little shoes that would have graced the feet of a Chinese lady, there could be seen gigantic boots, reminding one of seven-miles boots of the fairy tales, as well as specimens for the most varied deformities of the human foot. A certain kind of shoes and boots at first observation appeared a perfect riddle, but their distinctive feature was explained to consist in ventilating appliances.

The manufacture of hard caoutchouc is also extensive in France, owing

chiefly to the enterprising spirit of Charles Morey, who, having bought of Goodyear the exclusive privilege of the use of his invention for fifteen years, has shared it with many others willing to pay for it. Thus the French-American Company, which owns a large factory at Beaumont, in the department of *Seine et Oise*, possesses the exclusive right of manufacturing combs, for which production the first class medal of the French exhibition was awarded to it. The French company, which has factories at Lille and St. Dennis, has the exclusive right of manufacturing handles of knives and other cutlery, while the Goodyear company has alone the right of preparing all kinds of hard caoutchouc substitutes for whalebone in its various applications. Other privileges are similarly distributed. Charles Morey himself lately founded a factory at Metz, which produces a numberless variety of articles of caoutchouc, as well as gutta-percha.

The chaotic variety of articles manufactured in France of common or vulcanized caoutchouc and gutta-percha is almost bewildering. Elastic textures of every description, (silk, linen, cotton,) elastic stockings for persons suffering with the gout, gaiters, garters, suspenders, drawers; elastic bands, cords, belts, telegraph wires; aprons, window shades, carpets, gloves; stopples, bungs, diving apparatus, life-boats, bathing tubs; mattresses, pillows, tents; numberless articles for hunters, fishers, travellers, and photographers; utensils for the preservation of acids, bottles of every kind, cases, balloons, doll-heads, spinning cards, hurdles, troughs, pumps, umbrellas; these, and a thousand other objects, were shown at the Paris exhibition in the most charming disorder.

THE PRODUCTS

OF THE

COMBUSTION OF GUN-COTTON AND GUNPOWDER.

UNDER CIRCUMSTANCES ANALOGOUS TO THOSE WHICH OCCUR IN PRACTICE.

BY LIEUTENANT VON KAROLYI.*

REPRINTED FROM THE PHILOSOPHICAL MAGAZINE OF OCTOBER, 1863, WITH REMARKS BY DR. B. F. CRAIG.

THE gun-cotton manufactured according to Major General Freiher von Lenk's method at Hirtenberg, near Wiener Neustadt, has, in consequence of special previous experiments, been used by the Genie corps for mining purposes, and notwithstanding the fact that there are still numerous difficulties in the way of its use for gun charges, it is also used by the Royal Imperial Artillery for hollow projectiles.

The first-mentioned use led the Genie committee, to which I belong, to cause experiments to be made which are calculated to give greater insight into the chemical deportment of this substance. Among these is the attempt to ascertain the products of combustion of the gun-cotton produced in Hirtenberg; and in the course of the investigation it seemed advisable to extend the method I used to gunpowder.

I.—ANALYSIS OF THE PRODUCTS OF COMBUSTION OF GUN-COTTON.

The rapid deflagration of gun-cotton, and its necessary accompaniment, the bursting action, prevented me from using in the analysis of the products of combustion the excellent method which Professor Bunsen† devised for obtaining the combustion products of gunpowder for the purpose of analysis. It was necessary to effect the combustion *in vacuo*, and for this purpose I used a eudiometre about a metre in length, in which, instead of two wires, as in the ordinary eudiometre, a single very thin platinum wire was drawn across. To this from 15 to 20 milligrammes of gun-cotton were affixed, the tube filled with mercury, and the Torricellian vacuum produced in the usual manner. By means of a galvanic battery the wire could be ignited, and hence the gun-cotton exploded; thereupon all eudiometrical operations were carried out in the tube in the usual manner after a preliminary experiment had shown that

* Translated from Poggendorff's *Annalen*, April, 1863, by Dr. Atkinson, Royal Military College, Sandhurst.

† Phil. Mag., vol. xv, p 489.

the gas produced in this manner consisted of nitrogen, binoxide of nitrogen, carbonic acid, carbonic oxide, marsh gas, and aqueous vapor.

	Volume.	Pressure.	Temp.	Vol. at 0° and 1 m.
			°	
Original volume	374.53	0.1156	12	42.37
In the steam bath	415.83	0.1768	95	54.56
After absorption of NO^2	361.80	0.1078	11.2	37.47
After absorption of CO^2	328.06	0.0850	10.5	26.85
After addition of air	481.25	0.2372	12.3	109.26
After addition of oxygen	497.56	0.2510	12.5	119.41
After explosion	466.21	0.2212	11.2	99.07
After absorption of CO^2	430.57	0.1855	10.4	76.97
After addition of H	477.25	0.2301	11.7	105.29
After explosion	443.38	0.1983	12.6	84.08

The quantity of NO^2 and CO^2 is obtained from the absorptions, the quantity of water from the increase in volume in the steam-bath; the quantity of nitrogen is obtained from the volume 76.97, which remains after removing the carbonic acid resulting from the combustible gases, by subtracting the uncombined oxygen and the nitrogen contained in the atmospheric air added; while the combustible gases are calculated from the formulæ in Bunsen's gasometric method,

Carbonic oxide .... $=P_1-\frac{2P_2-P}{3}$

Marsh gas .... $=\frac{2P_2-P}{3}$

Hydrogen .... $=P-P_1$

in which P is the quantity of combustible gases, P_1 the carbonic acid produced in combustion, P_2 the oxygen used in combustion.

Hence the gases from gun-cotton contain in 100 parts—

	By volume.	By weight.
Carbonic oxide	28.55	28.92
Carbonic acid	19.11	30.43
Marsh gas	11.17	6.47
Binoxide of nitrogen	8.83	9.59
Nitrogen	8.56	8.71
Carbon	1.85	1.60
Aqueous vapor	21.93	14.28
	100.00	100.00

The gun-cotton used had the average composition $C^{24}H^{17}N^5O^{38}$, from which, after subtracting the results of the analysis, the separated carbon is obtained, which is included in the above analysis.

This simple and apparently faultless method has repeatedly shown that, by using a somewhat large quantity of gun-cotton under the same circumstances, when therefore the combustion takes place under comparatively greater pressure, the quantities of the products of combustion change, and the quantity of

binoxide of nitrogen diminishes as the pressure increases. Hence the deoxidation of nitrogen-compounds during the combustion takes place the more completely the greater the work which the gun-cotton has to perform during its combustion.

This circumstance suggested to me the idea of exposing the gun-cotton during its combustion to a determinate resistance, and regulated so that it just gives way at the moment the gun-cotton is completely burnt away. This condition led me to the experiment of placing a vessel filled with gun-cotton which offered the necessary resistance, in a 60-pr. mortar, which was then exhausted and the gun-cotton exploded by galvanism.

The resistance of the explosion vessels must be so chosen that the gas in the mortar, after explosion, has an excess of pressure of half an atmosphere, in order that it may subsequently be transferred to the measuring vessels.

The explosion vessels which I used were made according to the directions of the late Lieutenant Colonel Ebner, and consisted of hollow cast-iron cylinders closed at one end, while at the other was a nut through which the arrangement for a galvanic explosion passes. For this purpose the nut is provided with an excavation in which is a thin platinum wire fastened on the one hand to the insulated copper wire, and on the other to the copper wire which passes directly through the nut. Outside the cover the wires are bent into knots, which, as previously mentioned, serve to support the cylinders and to complete the voltaic circuit.

The weight of the gun-cotton whose gases shall fill the exhausted mortar of 5,216 cubic centimetres contents so that there shall be the tension above mentioned, I have empirically determined, and find that it is 10 grammes. The fact that 10 grammes of cotton somewhat compressed occupy a space of 10.5 centimetres in length and 2 centimetres in diameter, determined the internal dimensions of the cylinder. The thickness of the sides of the cylinder was also obtained from an empirical experiment, which showed that with a thickness of 8 millimetres the cylinder just exploded with production of flame, and that thus, in accordance with the condition stated, the gun-cotton burns away the moment the cylinder burst. I must here mention a peculiar circumstance which attracted my attention in determining the thickness of the side of the cylinder, and which serves to characterize gun-cotton. For the above investigation I successively filled with gun-cotton cylinders 4, 6, and 8 millimetres thick in the side and exploded them in a hole. Although the cylinders of 4 and 6 millimetres in thickness contained comparatively a larger charge, the pieces produced were considerably larger than those of the cylinder 8 millimetres in thickness. The former were often only split lengthwise, their cover and bottom remained unchanged, while the pieces of the cylinder of 8 millimetres in thickness were scarcely larger than hazel-nuts.

The above bursting vessels might also probably be constructed of glass. Very strong, thick glass tubes are taken, and at each end corks cemented in, one of which has been provided with a galvanic conduction and the small platinum wire. The length of the vessels and the thickness of their sides could then be regulated by the quantity of gas and the desired resistance.

The qualitative analysis of the products of the combustion of gun-cotton under the circumstances described gave carbonic oxide, carbonic acid, nitrogen, marsh gas, and a trace of a sulphurous gas, (probably a bisulphide of carbon compound,) which, from its small quantity, escaped analysis and could only be detected by the smell. This probably arises from a small trace of sulphuric acid adhering to the gun-cotton, which either was not removed in washing, or by subsequent treatment with potash remained as sulphate.

The quantitative gas analysis was made according to the following plan:

Absorption analysis.

	Volume.	Pressure.	Temp.	Vol. at 0° and 1 metre
			°	
Original volume	114.78	0.6242	19.1	66.94
After absorbing CO^2	84.88	0.6048	20.2	47.81

Combustion analysis.

	Volume.	Pressure.	Temp.	Vol. at 0° and 1 metre
			°	
Original volume	159.38	0.3144	19.8	46.72
After addition of air	238.48	0.4108	19.4	87.75
After addition of oxygen	293.77	0.4436	18.7	121.98
After explosion	248.16	0.3954	19.1	91.71
After absorption of CO^2	181.12	0.3504	19.5	59.19
After addition of H	251.65	0.4344	21.6	101.32
After explosion	174.85	0.3389	20.4	55.15

The quantity of carbonic acid obtained from the absorption analysis =19.13. The nitrogen found by known methods is 11.37 volume. The combustible gases are determined by the formula already mentioned in the case of the analysis in the Torricellian vacuum, as

Carbonic oxide .... $=P_1-\frac{2P_2-P}{3}=26.01$

Marsh gas .... $=\frac{2P_2-P}{3}=6.51$

Hydrogen .... $=P-P_1=2.83$

in which $P=35.35$, $P_1=32.52$, $P_2=27.44$. The gun-cotton used in the analysis corresponded to the formula $C^{24}H^{17}N^5O^{38}$, from which the water which cannot be directly determined, as well as the eliminated charcoal, may be found. Hence the mixture of gun-cotton gases consists of—

	By volume.	By weight.
Carbonic oxide	28.95	29.97
Carbonic acid	20.82	33.86
Marsh gas	7.24	4.28
Hydrogen	3.16	0.24
Nitrogen	12.67	13.16
Carbon	1.82	1.62
Aqueous vapor	25.34	16.87
	100.00	100.00

As previously mentioned, the bursting vessels were filled with 10 grammes of cotton, which, by an accurate measurement, was found to yield a quantity of gas of 5,740 cubic centimetres at 0° and 1 metre pressure. The contents of the mortar at 16° and 0.7382 metre pressure amounts to 5,292 cubic centimetres; the quantity of gas issuing at this pressure amounted to 2,939 cubic centimetres; hence 10 grammes gun-cotton yielded 8,231 cubic centimetres at 16° and 0.7382 metre pressure. If the quantity of gas is calculated from the results of the analysis it is found that 10 grammes of gun-cotton yield 5,764.2 of gases, which sufficiently agrees with the quantity actually found.

Comparing the results of the above described analysis with those of the analysis in the Torricellian vacuum, it is found—

(1.) That the gases in both cases are combustible from the large quantity of carbonic oxide they contain.

(2.) That the gases produced *in vacuo* contain a considerable quantity of binoxide of nitrogen, while by burning gun-cotton under appropriate resistance, the nitrogen compounds are deoxidized in favor of the carbon in marsh gas and of the hydrogen, by which an increase in the carbonic oxide, carbonic acid, water, and a separation of free hydrogen, are caused. Hence it follows that the red gun-cotton vapors can never occur if the entire gun-cotton is burnt away at the moment in which it begins to overcome the resistance offered to it.

These facts have a practical application in the use of gun-cotton to mining purposes.

II.—ANALYSIS OF THE PRODUCTS OF COMBUSTION OF GUNPOWDER.

After finding that the combustion of gun-cotton under circumstances resembling those which occur in mines is of decisive influence on the products evolved, it appeared desirable to investigate the combustion of gunpowder under similar circumstances. Unfortunately, since my investigations had a specifically military object, I could only analyze the Austrian small-arms and ordnance powder; hence only a superficial comparison can be instituted with the results which Professor Bunsen obtained with freely burning sporting powder.

The combustion of the gunpowder was effected in the exhaused mortar in the same way as the combustion of the gun-cotton, with the exception that, on account of the smaller action of the powder, and in order to obtain as large a quantity of gas, the exploding vessels had to be larger, but with thinner sides than those in which the gun-cotton was exploded. The excavation in the cores was filled with meal powder.

The composition of the two kinds of powder used for investigation are obtained from the following analysis:

Ordnance powder.—4.5487 grammes gave 3.3562 grammes saltpetre and 1.1923 gramme of a residue insoluble in water. Bisulphide of carbon dissolved 0.5823 sulphur. The remainder was charcoal.

Small-arms powder.—8.8653 grammes contained 6.8408 saltpétre; the residue of 2.0245 grammes contained 0.765 gramme sulphur, and there remained 1.2595 gramme of charcoal. The organic analysis of the charcoal, carefully freed from sulphur, gave—

For ordnance powder.

Carbon	81.200
Hydrogen	2.865
Oxygen	13.599
Ash	2.336
	100.000

For small-arms powder.

Carbon	82.90
Hydrogen	2.99
Oxygen	12.14
Ash	1.97
	100.00

Hence the percentage composition of both these kinds of powder is as follows:

Ordnance powder.

Nitrate of potash		73.78
Sulphur		12.80
Charcoal ..	Carbon	10.88
	Hydrogen	0.38
	Oxygen	1.82
	Ash	0.31
		100.00

Small-arms powder.

Nitrate of potash		77.15
Sulphur		8.63
Charcoal ..	Carbon	11.78
	Hydrogen	0.42
	Oxygen	1.79
	Ash	0.28
		100.00

The composition of the powder analyzed by Bunsen and Schischkoff was—

Nitrate of Potash		78.99
Sulphur		9.84
Charcoal ..	Carbon	7.69
	Hydrogen	0.41
	Oxygen	3.07
	Ash	0.00
		100.00

For the qualitative analysis of the products of combustion, two cylinders were filled with the two kinds of powder, made air-tight, and successively exploded in the mortar in the manner described.

For both kinds there were found in the solid residue: (1) sulphate of potash, (2) carbonate of potash, (3) hyposulphite of potash, (4) sesqui-carbonate of ammonia, (5) sulphur, (6) charcoal, (7) sulphide of potassium. The latter, in the case of the small-arms powder, was only formed in very small quantities.

The gaseous products of combustion were: (1) nitrogen, (2) carbonic acid, (3) carbonic oxide, (4) hydrogen, (5) sulphuretted hydrogen, (6) marsh gas, and a very small quantity of a bisulphide of carbon compound, which was distinctly recognized by its odor as being that produced in the gases from gun-cotton. The whole mixture is colorless, and contains no fume or vapor.

(a) *Ordnance powder.*—For the quantitative determination of the products of combustion 36.8366 grammes were used.

The gas passed into three absorption tubes amounted to 75.3 cubic centimetres; the gas issuing from the mortar until the rest was under the atmospheric pressure amounted to 5480.7 cubic centimetres at 16° C. and 0.749 metre pressure; under these circumstances the mortar holds 5216 cubic centimetres; hence the above quantity yielded 7621.96 cubic centimetres gas at 0° and 1 metre pressure.

The absorption analysis produced—

	Volume.	Pressure.	Temp.	Vol. at 0° and 1 metre pressure.
Original volume	90.72	0.6028	16.2°	51.63
After absorption of CO^2 and HS	53.71	0.5705	14.3	29.12

From the estimation of the potash bulb with iodine solution, it followed that the sulphuretted hydrogen corresponded to 0.44 division. Hence the above gas consisted of 0.44 vol. sulphuretted hydrogen, 22.07 carbonic acid, and of 29.12 nitrogen and combustible gases.

The explosion analysis of the gas freed from sulphuretted hydrogen and carbonic acid and transferred to the eudiometer was as follows:

	Volume.	Pressure.	Temp.	Vol. at 0° and 1 metre pressure.
Original volume	113.26	0.2729	15.6°	28.8
After addition of air	183.36	0.3494	16.8	60.36
After addition of oxygen	204.32	0.4295	16.4	71.79
After explosion	185.62	0.3522	15.4	61.89
After absorption of CO^2	167.90	0.3476	15.1	55.31
After addition of H	224.67	0.4068	16.2	86.30
After explosion	166.38	0.3355	15.7	52.79

By applying the formulæ of the gasometric methods already mentioned in the analysis of gun-cotton, since the gases are qualitatively the same, the values are obtained, for carbonic oxide = 5.21, hydrogen = 3.03, marsh gas = 1.38, and nitrogen = 19.18.

Hence the total gas calculated for 100 parts consisted of—

	Vols.
Carbonic acid	42.74
Sulphuretted hydrogen	0.86
Carbonic oxide	10.19
Marsh gas	2.70
Hydrogen	5.93
Nitrogen	37.58
	100.00

The determination of the solid residue in the mortar was effected, after removing the gases, by digestion with water, which was drawn off through a tap in the bottom, and rapidly filtered.

The results of the analyses were as follows:—

1. *Sulphide of potassium.*—The entire filtered liquid was digested in three large boiling flasks with well-ignited oxide of copper, thereupon filtered, and the residue dissolved in fuming nitric acid. Treated with nitrate of baryta, it gave 0.1015 grammes of sulphate of baryta, which corresponds to 0.0478 grammes sulphide of potassium in the residue of 36.8366 grammes powder.

The liquid filtered from the oxide of copper was made up to 6 litres for the sake of further investigation.

2. *Carbonic acid.*—A litre of this liquid gave with nitrate of silver a precipitate of carbonate and sulphide of silver. Treated with ammonia, the carbonate dissolved, and was separated from sulphide by a weighed filter, and precipitated in the filtrate as chloride of silver by means of hydrochloric acid. Its weight amounted to 3.0475 grammes, which corresponds to 0.4687 grammes combined carbonic acid; hence in the entire residue there were 2.8126 grammes combined carbonic acid.

As a control, the carbonic acid was determined by chloride of manganese by mixing a litre of the liquid with a solution of this salt which had been previously fused; a precipitate of carbonate of manganese was obtained, the carbonic acid of which, determined in the usual manner, corresponded to 2.8337 grammes in the entire residue.

3. *Hyposulphite of potash.*—The sulphide of silver (2) weighed on the tared filter, when dried at 120°, 0.2261 grammes, which corresponds to 0.1733 grammes hyposulphite of potash; hence the entire residue contained 1.0400 grammes hyposulphite of potash.

A determination of the hyposulphite was also made by metrical analysis. A litre of the solution was acidulated with acetic acid, mixed with starch, and determined by means of a standard solution of iodine. A litre required 22.57 cubic centimetres iodine solution; hence, according to the formula $s = a\frac{2KO, S^2 O^2}{I} = t$, in which $t = 22.57$, $a = 0.000517$; this litre contained 0.1746 grammes hyposulphite of potash, and the entire residue 1.0476 grammes of this salt.

4. *Sesquicarbonate of ammonia.*—According to Bunsen's method, a litre of the liquid was boiled with caustic potash, the distillate passed into a solution of hydrochloric acid of known strength, and the hydrochloric acid which had not been changed to chloride of ammonium determined with a standard ammonia solution. I found $a = 0.04853$, the quantity of hydrochloric acid taken; $t = 19.87$, the number of divisions of an ammoniacal liquid which would have saturated a volume of hydrochloric acid equal to that taken with $t' = 41.30$ divisions of the burette. Using the formula $x = \frac{2NH^4O,3CO^2}{2HCl}\frac{a(t'-t)}{t'}$, I found in the ¼ litre the sesquicarbonate of ammonia to be 0.041275 gramme; hence 0.9908 gramme corresponds to the entire quantity of sesquicarbonate of ammonia.

5. *Carbonate of potash.*—Subtracting the carbonic acid in the sesquicarbonate of ammonia = 0.5541 gramme from the total quantity found (2), = 2.8337 grammes, there remains a quantity corresponding to the carbonate of potash = 2.2796 grammes. Hence the entire residue contains 7.1498 grammes carbonate of potash.

6. *Sulphate of potash.*—A litre of the liquid mixed with chloride of barium gave 3.0244 grammes sulphate of baryta, which corresponds to 2.2683 grammes sulphate of potash for one litre of liquid, and 13.6100 grammes for the whole residue.

7. *Potash.*—To determine the entire quantity of potash contained in the various salts, a litre of the liquid was carefully evaporated to dryness with sulphuric acid and ignited in a platinum vessel. 3.8466 grammes of sulphate of

potash were thus obtained, corresponding to 2.0786 grammes of potash. Hence 100 grammes of ordnance powder contain 33.85 grammes of potash, which agrees closely with the result of the direct analysis of the powder. After finding, by direct observation, that 36.8366 grammes of the powder furnished 7621.9 cubic centimetres of gas, the composition of the products of combustion of this powder may be stated as follows:—

Sulphate of potash	13.61
Carbonate of potash	7.14
Hyposulphite of potash	1.04
Sulphide of potassium	0.04
Charcoal	0.94
Sulphur	1.73
Sesquicarbonate of ammonia	0.99
Nitrogen	3.60
Carbonic acid	6.40
Carbonic oxide	0.97
Hydrogen	0.04
Sulphuretted hydrogen	0.10
Marsh gas	0.15
Loss	0.07
	36.82

in which sulphur and charcoal are calculated from the deficiency.

(b) Small-arms powder.—34.153 grammes were used for the combustion. The quantity of the gaseous products was obtained from the following gasometric experiments:

Absorption analysis.

	Volume.	Pressure.	Temp.	Vol. at 0° and 1 metre pressure.
Original volume	136.94	0.6331	22.1°	80.21
After absorbing CO^2 and SH	75.04	0.5024	21.9	40.46

If the solution of the potash-bulb is determined with solution of iodine, it is found that 0.535 division corresponds to sulphuretted hydrogen, from which it follows that the above quantity of gas consists of—

Carbonic acid	39.22
Sulphuretted hydrogen	0.53
Combustible gases and nitrogen	40.46
	80.21

The explosion analysis with the transferred gas gave—

	Volume.	Pressure.	Temp.	Vol. at 0° and 1 metre pressure.
			°	
Original volume	120.12	0.3432	20.4	38.36
After addition of air	198.51	0.4263	20.3	78.77
After addition of oxygen	230.33	0.4478	20.5	95.89
After explosion	201.14	0.4323	19.6	81.47
After absorption of CO^2	189.46	0.4276	21.0	75.23
After addition of H	261.02	0.4817	21.2	116.66
After explosion	174.20	0.4130	18.3	67.43

If the formulæ previously used be applied, we get—

Carbonic acid	= 3.95
Hydrogen	= 5.24
Marsh gas	= 2.29
Nitrogen	= 26.88

Hence the small-arms powder contains in 100 volumes—

Carbonic acid	48.90
Sulphuretted hydrogen	0.67
Carbonic oxide	5.18
Marsh gas	3.02
Hydrogen	6.90
Nitrogen	35.33
	100.00

The solid residue in the mortar was treated with hot water and the liquid filtered. No sulphide of potassium was found to be present. The analysis was executed in the same manner as with the ordnance powder.

From the results of this analysis and from those of the gas analysis, direct measurement having shown that 34.153 grammes of ordnance powder give 7,738 cubic centimetres of gas, the following scheme for the products of combustion of the ordnance powder may be given:

Sulphate of potash	12.354
Carbonate of potash	7.096
Hyposulphite of potash	0.605
Charcoal	0.887
Sulphur	0.397
Sesquicarbonate of ammonia	0.908
Nitrogen	3.432
Carbonic acid	7.442
Carbonic oxide	0.504
Hydrogen	0.047
Sulphuretted hydrogen	0.079
Marsh gas	0 167
Loss	0.235
	34.153

The results hitherto obtained for the products of combustion of both kinds of powder may now be compared with each other, and also with those obtained by Bunsen and Schischkoff in the analysis of sporting powder:

I.—*Composition.*

		Sporting powder.	Small-arms powder.	Ordnance powder.
Nitrate of potash		78.99	77.15	73.78
Sulphur		9.84	8.63	12.80
Charcoal..	Carbon	7.69	11.78	10.88
	Hydrogen	0.41	0.42	0.38
	Oxygen	3.07	1.79	1.82
	Ash	0.00	0.28	0.31
		100.00	100.00	100.00

II.—*Gaseous products of combustion by volume.*

	Sporting powder.	Small-arms powder.	Ordnance powder.
Nitrogen	41.12	35.33	37.58
Carbonic acid	52.67	48.90	42.74
Carbonic oxide	3.88	5.18	10.19
Hydrogen	1.21	6.90	5.93
Sulphuretted hydrogen	0.60	0.67	0.86
Oxygen	0.52	....	....
Marsh gas	....	3.02	2.70
	100.00	100.00	100.00

III.—*Total products of combustion by weight.*

	Sporting powder.	Small-arms powder.	Ordnance powder.
Sulphate of potash	42.27	36.17	36.95
Carbonate of potash	12.64	20.78	19.40
Hyposulphite of potash	3.27	1.77	2.85
Sulphide of potassium	2.13	....	0.11
Sulphocyanide of potassium	0.30	....	....
Nitrate of potash	3.72	....	....
Charcoal	0.73	2.60	2.57
Sulphur	0.14	1.16	4.69
Sesquicarbonate of ammonia	2.86	2.66	2.68
Nitrogen	9.98	10.06	9.77
Carbonic acid	20.12	21.79	17.39
Carbonic oxide	0.94	1.47	2.64
Hydrogen	0.02	0.14	0.11
Sulphuretted hydrogen	0.18	0.23	0.27
Oxygen	0.14	....	....
Marsh gas	....	0.49	0.40
Loss	....	0.68	0.19
	100.00	100.00	100.00
Quantity of gas for a gramme of powder	190.00	226.59	200.91

A comparison of these results shows at first sight that, on the whole, the products of combustion of powder are little dependent on the manner in which the combustion takes place. But that the composition of the powder has a great influence, is seen from the fact that in Bunsen's powder, which contains much nitre, nearly 4 per cent. of this substance are found in this residue; while, on the other hand, in the residue of the ordnance powder, which contain less nitre, almost seven per cent. of sulphur and charcoal are separated unburnt. The influence of the composition on the nature of the products of combustion is still

more surprising. Where the reducing body preponderates, the combustion of the carbon is more imperfect. Whereas the gases of sporting powder only contain three per cent. of carbonic oxide, the gas from ordnance powder contains nearly ten per cent. The quantity of hydrogen and of marsh gas increase in the same direction, so that the ordnance powder gas contains nearly twenty per cent. of combustible gases. Hence it is not surprising that the gases of ordnance powder, as well as those of gun-cotton, may be ignited, as direct experiment showed, by a glimmering piece of wood.

There might apparently be no difficulty, from the results of these analyses, in arriving at a right composition of powder; yet in this respect practice prefers its own empirical path. But in any case the results obtained serve as an additional proof of the inaccuracy of the view which prevails in many chemical text-books and in almost all artillery institutions—that powder must decompose, in burning, into sulphide of potassium, carbonic acid, and nitrogen. If practice has no other reason for the composition of powder than the possibility that these products may occur, it is certainly allowable to attempt to prove experimentally that the products of combustion, even under the circumstances which prevail in practice, can never be formed alone, and that, indeed, one of them—sulphide of potassium—in many cases is not formed at all.

DR. CRAIG'S REMARKS.

It will be seen from the foregoing that Lieutenant von Karolyi burnt gun-cotton under two conditions, and determined for each the composition of the resultant gases. In the one case he ignited a small quantity by means of a voltaic current in an eudiometre which had been exhausted of air by the Torricellian method, and in which, consequently, the cotton burned under very small pressure. In the other, an iron cylinder was filled with gun-cotton, placed in an exhausted vessel, and ignited in a similar manner, so that the combustion went on under pressure until the enclosing tube was broken. This increase of pressure was found to give rise to a modification in the composition of the resultant gases; and, for purposes of comparison, the results in the two cases may be expressed in chemical symbols, with numbers affixed, which give with sufficient accuracy the relative amounts in volume.

Gas produced.	Burning without pressure.	Burning under pressure.
N	85	130
HO	220	250
CO	285	290
CO_2	190	210
NO_2	90	......
CH_2	110	70
H	......	30

The interesting experiments of Mr. Abel on the combustion of gun-cotton in the receiver of an air-pump exhausted to different degrees point to the conclusion that, in burning, gun-cotton is decomposed by the action of heat into certain products, among which are the binoxide and some of the higher oxides of nitrogen, and a large quantity of combustible gases. These gaseous products react on each other with the disappearance of the oxides of nitrogen and the production of new compounds if their temperature is maintained above a certain point, but if they are allowed to expand into a vacuum as fast as they

are generated they cool so that their mutual action is prevented. The amount of reaction between the first products of decomposition will depend on their temperature and on the time during which they are maintained at that temperature.

The experiment of bursting a loaded shell cannot be said to present the same circumstances as those which prevail when the explosive material is used in a fire-arm; for, in the first place, that part of the charge which burns after the rupture of the shell and the consequent expansion of the contained gases does not burn under pressure; and, in the second place, the products even of that portion of the combustion which antecedes the rupture of the shell are not kept together at a high temperature as long as they are in the barrel of a gun. If the cylinder used by Lieutenant von Karolyi had been of such strength as to resist bursting, and the gases have been allowed to escape into the exhausted vessel through a small vent, the circumstances would have been more nearly approximated to those which occur in practice with fire-arms.

For the purpose of determining what is actually produced in the case of the firing of a gun, the most satisfactory plan would seem to be to screw the muzzles of a number of loaded musket barrels into a sufficiently strong and air-tight vessel, and, their vents being securely closed and the whole apparatus exhausted by the air-pump, to fire them in succession by the galvanic current, and then examine the products according to the method followed by Lieutenant von Karolyi.

In the barrel of a gun, especially when loaded with a heavy projectile, the products of combustion remain under pressure and at a high temperature for a comparatively long time, and the products of the explosion of Lieutenant von Karolyi's cylinders are such that, under these conditions, we would expect them to decompose each other. Thus in the case of gunpowder, sulphate of potash was produced, together with unconsumed charcoal, sulphur, and various combustible gases. Now we know that sulphate of potash, kept at a red heat in contact with such reducing agents, will part with its oxygen, and be converted into the sulphide.

Lieutenant von Karolyi finds mere traces of the sulphide of potassium among his products, but, on the other hand, the residue left in the barrel of a fire-arm after its discharge is found to consist mainly of this salt. This circumstance does not show conclusively that sulphide of potassium is the chief solid product of the explosion, for it may have a special tendency to accumulate on the walls of the gun, being deposited on the cold metal from a state of vapor; but the amount of its deposition makes it probable that it constitutes no inconsiderable proportion of the products of combustion.

Some years ago Captain Rodman made certain experiments, in the course of which he exploded gunpowder in shells of great strength, which had in them small vents through which the resultant gases could make their escape; this escape, however, requiring a measurable length of time. I obtained, by the kindness of Captain Rodman, some of the solid residue left in the shell after these explosions; but, when it reached me, it was in a moist and deliquesced condition, and had apparently suffered decomposition by exposure to the atmosphere. When acted upon by dilute hydrochloric acid it evolved a large amount of carbonic acid gas, and when treated with distilled water and the liquor filtered, small precipitates only were produced by acid solutions of chloride of copper and of chloride of barium, so that but little sulphur was present either as sulphide or as sulphate. The only way in which I could account for such a condition of things was by supposing that the mass had originally contained sulphur in combination as sulphide of potassium, and that this, by the action of the moisture and carbonic acid of the atmosphere, had been transformed into carbonate.

In comparing the results of the combustion of gun-cotton in a vacuum with those produced by the explosion of an iron cylinder filled with it, it will be perceived that the change from one series of products to the other involves an increase in the volume of the evolved gases—an effect due chiefly, but not wholly, to the heat produced by the chemical reaction between the nitric oxide generated by the first act of combustion and the carburetted hydrogen present.

When gun-cotton, therefore, burns in a sufficiently strong and well-filled vessel, it is resolved into gaseous products which immediately react on each other with an increase in the temperature and tension of their mass, and on the suddenness of this reaction is probably due some part of the great percussive force developed by the explosion of gun-cotton in strong vessels.

I believe that no determination has been made of the amount of heat evolved by the explosion of gun-cotton.

The sums of the heat, and of the mechanical effects representing heat, produced by equal weights of gun-cotton and of gunpowder would not be very different if assumed to be proportional to the amounts of oxygen concerned in the chemical reactions in each case, but the greater volume of the gases evolved from gun-cotton makes their actual temperature less and their mechanical effect greater.*

The much greater heating effect, however, which gunpowder exerts upon the gun from which it is fired is to be attributed not only to the higher temperature and greater density of the products of explosion, but to the circumstance that in the case of gunpowder sulphide of potassium is deposited on the walls of the gun, probably from a state of vapor, imparting to the cold metal both its free heat and its heat of condensation, the action being analogous to that of steam, which, in condensing on a cold body, heats it much more rapidly than a current of a non-condensible gas of the same temperature could have done.

B. F. CRAIG.

* It may be here remarked that the comparative mechanical energy developed in fire-arms by gunpowder and by gun-cotton is to be estimated not by the amount of motion imparted to the projectile, but by that imparted to the gun.

These two are different things, and the latter must always be greater than the former by an amount equal to the *vis viva* with which the products of decomposition of the projecting agent are expelled from the gun, and this *vis viva* must, of course, vary with the weight of the explosive material.

This consideration makes it evident why, when a lesser weight of gun-cotton is substituted for a greater weight of gunpowder, the recoil of the gun is less, while the velocity of the shot may be unchanged. B. F. C.

DESCRIPTION OF APPARATUS FOR TESTING

THE

RESULTS OF PERSPIRATION AND RESPIRATION

IN THE PHYSIOLOGICAL INSTITUTE AT MUNICH.

BY PROFESSOR MAX PETTENKOFER.

TRANSLATED FROM THE GERMAN BY PROFESSOR A. TEN BROOK.

In order to determine the quantity of carbonic acid and water secreted by the skin and lungs, various methods have been proposed. The methods of Scharling, Vierordt, Valentin and Brunner, Regnault and Reiset, Smith and others, with their results, are sufficiently known to every physiologist and chemist. What has been justly objected to all methods hitherto applied to men and the larger animals has been in reference essentially to two considerations: first, that the degree of accuracy of the methods had not been ascertained by test-experiments with known quantities of carbonic acid; and second, that men and animals had been forced to respire under conditions more or less unusual or oppressive, and hence not natural. I have, therefore, for years been occupied with the thought of some method for determining with sufficient precision the quantity of carbonic acid which a man develops when moving and breathing freely without the interposition of any apparatus whatever. The investigations of Bischoff and Voit in regard to the nourishment of carnivorous animals have shown that the carbonic acid passing off through the skin and lungs cannot be with certainty calculated from the difference between the carbon taken in with the food and that eliminated in the urine and excrement, taking into account also the weight of the body, because two unknown things, carbonic acid and water, escape at the same time and in varying proportions, through the skin and lungs. Since, then, there was a necessity for determining directly one at least of the two quantities, I resumed the attempt at a solution of the problem. I soon perceived that success was attainable only by directing over the human body a current of air of measured and constant force, and then determining the accession of carbonic acid and water to this current of air. It very soon struck me that something like a parlor stove might be adopted as a model. As long as the chimney draws, no smoke escapes at the joints or door of the stove, but the outside air presses from all directions into the stove in order to reach the chimney. If an exact measurement, in the pipe which conducts the smoke from the stove to the chimney, of the amount of air which moves in it is possible; if, further, the composition of the air which enters the stove and passes from it can be ascertained from a portion of it, then all the factors are obtained which are needed in order to determine what admixture the air-current receives from the process of combustion in the stove. In the plan which I devised the stove is represented by a small chamber of sheet-iron, which I shall call the saloon, placed within a larger apartment; the former being eight Bavarian feet in extent each way, with light admitted from the top and through windows on the sides. The windows should be cemented and the walls and ceiling riveted as nearly as possible air-tight. The door has movable openings, in order, when necessary, to render the admission of air practicable at other points than the joints of the

door. On the side opposite to the door, two openings, one below and the other above, lead through two conducting pipes outside the small chamber into a single and larger pipe, through which the air flows towards that portion of the apparatus which performs the functions of a draught-chimney. This portion, which may be placed in another apartment of the house than that in which the iron-chamber stands, consists of two suction-cylinders with hinge-valves, which may be equally moved by a strong clock-work, with any desired force, by means of a small steam apparatus. The falling weight of the clock-work is at each moment wound up again as fast as it descends. In this way a constant current of air through the door of the iron-chamber to the suction-pumps can be maintained. The air cannot, however, reach the suction-pumps without passing through a measuring apparatus which is in constant operation. For this purpose I have chosen a gas-clock, or stationary gas-meter, of such dimensions that 3,000 English cubic feet per hour can be accurately measured by it. In order to examine a portion of the air which enters through the door and other apertures of the apparatus-room and passes out of the same by the united conductor to the gas-meter, and from the ascertained difference in the amount of water and carbonic acid, to be able to reckon the quantity which was added inside of the apparatus, two aspirators* are employed, which draw uniformly each a constantly equal portion of air. The water of the air is, after the known method, absorbed by sulphuric acid and weighed; the carbonic acid is ascertained by means of the air rising in small bubbles through a determinate quantity of lime-water of known strength, and the lime-water finally is examined in regard to its amount of quicklime by treatment with diluted oxalic acid, exactly as I have described on another occasion.

In order finally to be able to test the air which remains behind in the iron-chamber of the apparatus, a suction and force pump is placed in connexion with the pipe which conducts the air away, by the aid of which flasks holding six to eight litres can be filled with air, which can be tested in regard to its amount of carbonic acid by lime-water. The same pump serves also to ascertain, at suitable times during the experiment, the fluctuations of the carbonic acid in the air current. In addition to this, there is an arrangement which permits the making trials, of any number and extent, without suffering any loss of air for the measurement of the entire current. A flask is attached air-tight to the pump, the air of which flask is perfectly replaced by continued pumping with air from the conductor. The air forced from the flask is not allowed to escape, but is conducted by an India-rubber tube back again into the current which passes on to the gas-meter to a point, of course, where the determination of the carbonic acid cannot be affected by it; there is, therefore, a flask of air introduced below, and in its place a flask of air withdrawn from the apparatus. In order that the current of air may not discharge any water by evaporation from the large gas-meter, the air passes, before it enters the gas-meter, through a standing cylinder filled with pieces of pumice-stone, which are to be kept moist. Where the air passes out of the moistening apparatus a psychrometer is placed in the pipe in order to show the temperature and moisture with which it enters the gas-meter, and is measured. There are also in the conductor in front of the moistening apparatus a psychrometer and several places for attaching tubes in order to take out air for experiment.

After communicating the plan which I had projected to the president of the academy, Baron Von Liebig, and a few other associates in the same branch of science, I applied to the technical commission of the natural sciences of the academy. Upon a report of this commission, accompanied by accurate estimates of costs, his Majesty granted out of his private purse 4,000 florins for the construction of the apparatus. I but follow the dictates of my heart, and

* These aspirators are now replaced by two small pumps, which act both by suction and pressure.

what would be the sense of duty of all who deem the physiology of assimilation important, when, on this occasion of making to the academy my first report of the apparatus now completed and tested, I express my most profound emotions of gratitude to his Majesty King Max II, of Bavaria, as the generous protector and intelligent promoter of the sciences.

The whole apparatus was put up during the last winter. Since May I have occupied myself with testing it in every respect, and can now declare it complete and entirely satisfactory for the purposes proposed, which may be said also of all the methods of investigation employed in using it. That upon which the whole finally turned was the proof that the amounts of carbonic acid developed in the saloon of the apparatus could really be found again and determined with the requisite exactness, a test which in all previous respiratory apparatus has been wanting. After I had by various experiments ascertained all the influences which the apparatus and the methods exert upon the exactness of the results, I selected some stearine candles of good quality and determined their amount of carbonic acid by elementary analysis. They yielded on the average, after three concurring experiments, (*Verbrennungen,*) for which the material had always been taken from a different candle, to 100 parts by weight, 291 parts by weight of carbonic acid, so that to one grain of stearine may be computed 1,484 cubic centimetres of carbonic acid, the weight of a litre of carbonic acid at 0° C. and 760 millimetres quicksilver pressure being reckoned at 1.987 grammes. If the suction-pumps of the apparatus and the aspirators for the analysis of the air were in operation at the same time, a weighed candle in the saloon was lighted from without and before the close of the experiment again extinguished from without and afterwards weighed, the carbonic acid formed by the burning of the candle must be partly in the air which has passed through the large gas-meter and partly in that which remains behind in the saloon. The amount of carbonic acid in the air which goes through the gas-meter is ascertained, as already mentioned, by passing through lime-water as long as the air flows and is measured, a constantly equal portion (say, 100 cubic centimetres per minute) drawn without interruption from the current which passes from the saloon to the gas-meter. The amount of carbonic acid which remains behind in the air of the saloon is determined in this manner: after a proper mixing of air-strata in the saloon by means of a fan put in motion from without, two or more flasks of six to eight litres, filled by the pump at the conductor leading off from the saloon, are tested with lime-water and an estimate made founded upon the known cubic measure of the saloon. Not until after these flasks are filled should the saloon be entered to take out and weigh the candle. As, however, the air which has passed through the gas-meter and that which has remained behind in the saloon contains not only the carbonic acid which arose from the burning of the candle in the saloon, but also that part which the air already contained when it entered the saloon from without, the amount of carbonic acid contained in the entering air must be deducted. This may be known from repeating the experiment by which the air flowing in is drawn off and examined in exactly the same manner and as nearly as possible the same quantity as that passing out. Only the difference, therefore, of the carbonic acid within and without is reckoned, and it is precisely this which makes the determinations exact, since all the constant errors of the system are thereby eliminated.

It is obvious that all measured quantities of air must be reduced, as regards tension of vapor, temperature and air-pressure, to the usual standard.

I do not venture to ask the attention of the reader to all the necessary details of the apparatus or of an experiment. I must reserve these and their justification to a more extended discussion in the papers of the technical commission, and take the liberty here only briefly to state, in addition, the results of three quantitative experiments.

I.

During an experiment which lasted 184 minutes, 25.210 grs. of a stearine candle were consumed, which must generate 36.921 litres of carbonic acid. During the time of the experiment 4.9722 litres of air passed through the gas-meter. As the difference of the carbonic acid of this air and that entering the apparatus from without, there results, in place of the above amount, 31.623 litres of carbonic acid. There were still 5.922 litres of carbonic acid remaining in the saloon, and hence there was found 0.6 litre, or 1½ per cent., as surplus.

II.

The experiment lasted 215 minutes; 33.776 grs. of stearine candle were consumed, which represent 49.510 litres of carbonic acid. 58.554 litres again passed through the gasmeter, together with 41.690 litres of carbonic acid. 8.019 litres of carbonic acid remained still in the saloon. There were, therefore, found 0.19 litre of carbonic acid, or about 0.4 per cent. too much.

III.

The experiment lasted 188 minutes; 27.513 grs. of stearine candle were consumed, representing 40.298 litres of carbonic acid. 50.680 litres of air passed through the gas-meter, together with 33.347 litres of carbonic acid; in the saloon remained still 7.328 litres of carbonic acid. There were found, therefore, 0.277 litre, or 0.6 per cent. too much.

It will be perceived that the result of the experiments agrees very nearly with the theory; better, indeed, than could have been expected in view of the large dimensions of the apparatus and the great rarefaction of the carbonic acid. The accuracy is at least fully sufficient for the purpose proposed; and by other experiments I have been convinced that the determination of the carbonic acid remaining in the saloon is the main source of the slight uncertainty which still occurs, as this cannot be drawn off with the desirable exactness. If the carbonic acid remaining in the saloon amounts to more than one-fifth of the quantity which is contained in the current which had passed through the gas-meter, the uncertainty becomes very perceptible, and may amount in case of one-third and over to even seven or eight per cent. As proof of this I adduce still two other trials affected with this error.

(a.) The experiment lasted 157 minutes; 21.485 grammes of stearine candle were consumed, answering to 31.465 litres of carbonic acid. 42.862 litres of air passed through the gas-meter, together with 21.56 litres of carbonic acid. In the saloon remained still 7.57 litres of carbonic acid, or 5¾ per cent. too much.

(b.) The experiment lasted 108 minutes; 16.129 grs. of stearine candle were consumed, representing 23.621 litres of carbonic acid. 29.626 litres of air passed through the gas-meter, together with 15.02 litres of carbonic acid. In the saloon remained still 6.73 litres of carbonic acid. There was found, therefore, 8 per cent. too little.

Supported by these and still other experiments, I can with safety assume that, in an experiment of such duration, more than four-fifths of the carbonic acid developed passes over into the current between the saloon and the gas-meter; no greater uncertainty than one or at most two per cent. is to be feared. As in experiments with men and animals the time may be extended to 12 and 24 hours, the hope of attaining a still greater accuracy is not unfounded. I should have been pleased to extend a test experiment with candles to 24 hours; the aspirators for the examination of the air, however, which are now at my disposal, perform their functions only five hours without interruption. A remedy will be found for the defect within a short time in a small pump apparatus, which, in connexion with the large suction pumps in the engine-house, will constantly

furnish a uniform portion of the air inside and outside of the apparatus for examination, so long as the air current is in motion—that is, so long, in general, as an experiment continues.*

In closing I take the liberty to call attention specially to the fact that the respiratory and perspiratory apparatus in the Physiological Institute here is the first in which a result is possible under normal conditions; persons can live in it just as in a well-aired room, in which they can freely move, labor, eat and sleep, as they had been accustomed to do. By a movable window at the door of the saloon, food and other things can be taken in and out, without the fear of disturbing the experiment, with just as little concern as in a room, supposing the chimney-draught in order, one opens the door to stir the fire or remove the ashes without the escape of smoke.

The person outside of the saloon, conducting the experiment, does not in the least disturb the result by his respiration, &c., for the amount of carbonic acid of the air entering the saloon is constantly controlled by one of the two examining apparatus, and can therefore be drawn off. I have never hesitated to smoke cigars during the progress of a test experiment, or to receive visitors who also smoked, knowing that the changes of the air outside of the saloon are to be ascertained precisely in the same way and with the same exactness as those in the saloon. As only the difference is calculated, it is all the same whether this is more or less, provided it can be determined with certainty.

In the test experiment with candles I have hitherto employed a change of air of somewhat more than eleven English cubic feet (about 314 litres) per minute. In an hour, therefore, there entered into the saloon, which contains somewhat more than 12,000 litres, much more than its own capacity of fresh air. By increasing the force of the suction-pumps which are worked by the engine, the air-change can be quadrupled without thereby producing the slightest sensible draught in the saloon, except in the immediate vicinity (four to six inches) of the openings in the saloon door. Opposite to these openings the transverse section of the saloon is so considerable, that the rapidity of the movement of the air must become imperceptible in the saloon itself, even if it is felt immediately at the narrow openings. Under the greatest force of the suction-pumps, which answers to a ventilation of 3,000 English cubic feet an hour, a candle still burns perfectly undisturbed in the middle of the saloon.

That the rapidity of the entrance of the air at the saloon door is greater than that of its diffusion—in other words, that there is no loss of carbonic acid to be feared from the diffusion—is established simply by noticing whether the pungent smelling smoke generated and observed in the saloon is observable at the cracks on the outside. After this experiment had been repeatedly made with negative results, one might have been *a priori*, satisfied that no carbonic acid developed in the saloon can be lost, which fact is also perfectly established by the quantitative determinations. I am convinced that with this apparatus all questions of animal and vegetable physiology, so far as they relate to an increase or diminution of carbonic acid and water in the air, can be solved with exactness and under perfectly natural conditions.†

NOTE BY THE TRANSLATOR.—Professor Pettenkofer handed me the above report, at my request, when I once visited him during an experiment in the Physiological Institute, and he then made in pencil-mark the several short notes which are given in connexion with the translation. A. T. B.

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* The test experiments with candles have since been extended to twelve hours, and have given an entirely accurate result. As already stated, the pump apparatus connected with the propelling machinery provides, at present, for the examination of the two portions of air, (*Luftproben.*)

† These anticipations have all been entirely realised, since November, 1860, by experiments, as well upon men as upon animals.

THE SOLAR ECLIPSE OF JULY 18, 1860.

BY DR. J. LAMONT.

Translated from "Fortschritte der Physik," xvi, pp. 569–602, by Professor J. S. Hubbard, United States Navy.

A.—LIST OF PREPARATORY MEMOIRS.

(1.) *Secchi.*—Sulla eclisse solare del 18 luglio 1860, discorso lette alla Pontificia Accademia Tiberina, con note. Rome, 1860, printed from Giornale Arcadico CLXIV.

(2.) *Lamont.*—Jahresbericht der Münchner Sternwarte für 1858.

(3.) *Lamont.*—Letter to Professor Airy. Monthly Notices, xx, 93.

(4.) *Hind.*—Nautical Almanac. Circular, No. 5.

(5.) *Airy.*—On the observation of the solar eclipse, July 18, 1860. Monthly Notices, xx, 181.

(6.) *American Nautical Almanac.*—Total eclipse of July 17, 1860. Specially printed.

(7.) *Mädler.*—L'eclipse solaire du 18 Juillet, 1860. Observations of Imperial University Observatory, Dorpat. xv, Part I, App., p. 1.

(8.) *Faye.*—Sur l'eclipse solaire du 18 Juillet, 1860. Comptes Rendus xlviii.

(9.) *Aguilar.*—Sobre el eclipse total de sol que tendrá lugar el 18 de Julio de 1860. Annuaire of Royal Observatory of Madrid, 1860, p. 152.

(10.) *Carrington.*—An eye-piece for the solar eclipse. Monthly Notices, xx, 189.

(11.) *A. Thomson.*—On the importance of making observations of thermal radiation during the coming eclipse of the sun. Monthly Notices, xx, 317.

(12.) *K. von Littrow.*—Andeutungen über astronomische Beobachtungen bei totalen Sonnenfinsternissen. Wien. Ber., xxxix, 625.

(13.) *Bache, A. D., and Gilliss.*—Proposed expeditions to Cape Chudleigh and Steilacoom to observe the total eclipse of July 18, 1860. Monthly Notices, xx, 318.

(14.) *D'Abbadie.*—Letter relating to the observation of the solar eclipse, July 18, 1860. Monthly Notices, xx, 189.

(15.) *Rico y Sinobas.*—Selection of stations in Spain. Monthly Notices, xx, 102; Comptes Rendus, l, 33.

(16.) *Wolfers.*—Die totale Sonnenfinsterniss am 18 Juli, 1860. Astr. Nachr., xlviii, 33.

(17.) *E. Edlund.*—Ueber die Polarisation des Lichtes der Corona bei totalen Sonnenfinsternissen. Astr. Nachr., lii, 305.

(18.) *Heis.*—Die Sonnenfinsterniss vom 18 Juli, 1860. Heis, W. S., 1860, No. 26.

(19.) *C. Haase.*—Die Sonnenfinsterniss. Populär beschrieben, nebst erläuternden Angaben für die totalen Finsterniss am 18 Juli, 1860. Hannover.

(20.) *A. M. Nell.*—Die totale Sonnenfinsterniss am 18 Juli, 1860. Mainz.

(21.) *Hirsch.*—Vorausberechnung der totalen Sonnenfinsterniss vom 18 Juli, 1860. Vienna, 1855.

(22.) *Von Feilitzsch.*—Ueber physikalische Erscheinungen bei totalen Sonnenfinsternissen. Peters' Zeitschrift für populäre Mittheilungen, i, ii.

B.—OBSERVATIONS AND THEIR RESULTS.

(23.) *Le Verrier and L. Foucault.*—Eclipse du 18 Juillet, 1860. Rapport à son Exc. le ministre de l'instruction publique. Cosmos, xvii, 145–150, 173–183, 201–209; Inst., 1860. pp. 225, 226, 263, 264, 271, 272; Heis, W. S., 1860, pp. 253, 254, 260, 261, 267–269, 277–280, 286, 287, 289–298, 310–312; Cimento, xii, 32–32; Bulletin de l'Obs. de Paris, 4–7 Sept. 1860.

(24.) *Chacornac.*—Description des objets lumineux en déhors du disque solaire pendant l'eclipse totale du 18 Juillet, 1860. Bull. de l'Obs. de Paris, 4–8, Sept., 1860.

(25.) *Burat.*—Observation de l'eclipse totale du soleil du 18 Juillet, 1860. Bulletin de l'Obs. de Paris, 26–27 Sept., 1860.

(26.) *G. D. Weyer.*—Ueber die totale Sonnenfinsterniss vom 18 Juli, 1860. Kiel, 1860, pp. 1–28.

27. *Sidler.*—Die totale Sonnenfinsterniss am 18 Juli, 1860. Communications of Naturforschende Gesellschaft in Bern, 1860, pp. 146–152.

(28.) *Mannheim.*—Franges mobiles incolores observées pendant l'eclipse du soleil du 18 Juillet, 1860. Annales de chimie, (3,) lx, 207–210; Heis, W. S., 1861, pp. 87, 88.

(29.) *Faye.*—Sur les franges d'interférence qui se sont montrées en Algérie durant l'eclipse solaire du 18 Juillet, 1860. Comptes Rendus, li, 999–1002; Cosmos, xvii, 758–761; Institut, 1861, pp. 5, 6.

(30.) *Bremiker.*—Bericht über die Beobachtung der Sonnenfinsterniss vom 18 Juli, 1860. Berliner Monatsbericht, 1860, pp. 693–708; Heis, W. S., 1861, pp. 139–142.

(31.) *Bruhns.*—Observations de l'eclipse solaire à Moncayo. Cosmos, xvii, 230, 231; Heis, W. S., 1861, pp. 159, 160, 163–165; Bulletin de l'Obs. de Paris, Aug. 20–21, 1860; Astr. Nachr., liv, 305.

(32.) *Klinkerfues.*—Ueber die Beobachtungen der Sonnenfinsterniss vom 18 Juli, 1860, in Spanien. Götting Nachr., 1860, pp. 342–344; Astr. Nachr., liv, 263.

(33.) *Mädler.*—Ueber die totale Sonnenfinsterniss vom 18 Juli, 1860, beobachtet zu Vittoria. Tagebl. d. Naturf. in Königsberg, 1860, pp. 44–45; Zeitschrift fur Naturwissenschaft; xvi, 466, 467.

(34.) *Mädler.*—Ueber totale Sonnenfinsterniss mit besonderer Berücksichtigung der Finsterniss vom 18 Juli, 1860. Jena, 1861.

(35.) *Von Parpart.*—Beobachtung der partiellen Sonnenfinsterniss zu Storlus. Astr. Nachr., liii, 331.

(36.) Beobachtung der partiellen Sonnenfinsterniss an der kais. kgl. Marine-Sternwarte in Triest. Astr. Nachr., liii, 339.

(37.) *Dembowski.*—Beobachtung der partiellen Sonnenfinsterniss zu Mailand. Astr. Nachr., liii, 343.

(38.) *Reslhuber.*—Beobachtung der partiellen Sonnenfinsterniss zu Kremsmünster.

(39.) *Schmidt.*—Beobachtung der partiellen Sonnenfinsterniss in Athen. Astr. Nachr., liv, i.

(40.) *Galle.*—Beobachtung der partiellen Sonnenfinsterniss in Breslau. Astr. Nachr., liv, ii.

(41.) *Goldschmidt.*—Observations de l'eclipse de soleil du 18 Juillet, (Vittoria.) Comptes Rendus, li, 265–268; Institut, 1860, pp. 265, 266; Heis, W. S., 1860, p. 319, 320, 323–325.

(42.) *Goldschmidt.*—Die totale Sonnenfinsterniss vom 18 Juli, 1860, beobachtet zu Vittoria. Astr. Nachr., lvi, 805.

(43.) *Bianchi.*—Note sur l'eclipse totale de soleil observée à Vittoria le 18 Juillet, 1860. Comptes Rendus, li, 223.

(44.) *Von Feilitzsch.*—Indication des faits observées à Castellon de la Plana, royaume de Valence, Espagne. Comptes Rendus, li, 229–232; Institut, 1860, pp. 277, 278; Cosmos, xvii, 229, 230.

(45.) *Von Feilitzsch.*—Beobachtung der totalen Sonnenfinsterniss vom 18 Juli, 1860, in Castellon de la Plana. Astr. Nachr., liv, 81.

(46.) *J. N. Legrand.*—Sur l'eclipse totale du 18 Juillet, 1860. (Castellon de la Plana.) Comptes Rendus, li, 268, 269.

(47.) *Plantamour.*—Observation de l'eclipse totale de soleil du 18 Juillet, à Castellon de la Plana. Archiv d. sc. phys., (2,) viii, 311–322; Institut, 1860, pp. 315–318.

(48.) *Plantamour.*—Eclipse solaire du 18 Juillet, 1860. Comptes Rendus, li, 608–613.

(49.) *Airy.*—Account of observation of the total solar eclipse of 1860, July 18, made at Hereña. Monthly Notices, xxi, i; Arch. d. Phys., (2,) xi, 311–315.

(50.) *Faye.*—Sur l'eclipse totale du 18 Juillet dernier et sur les observations de Mr. Plantamour. Comptes Rendus, l, 378–386; Cosmos, xvii, 326–329; Heis, W. S., 1860, pp. 336–338; Zeitschrift für Naturw., xvi, 468–471; also a note, C. R., li, 708, 709.

(51.) *Brazmowski.*—Observation de l'eclipse totale de soleil du 18 Juillet, 1860, à Briviesca, Espagne. Comptes Rendus, li, 195–197.

(52.) *Brazmowski.*—Causes des rayons courbes de la couronne des eclipses solaires. Cosmos, xvii, 748, 749.

(53.) *Liais.*—Sur la polarisation de la couronne des eclipses. Pointillé du soleil observé au zenith. Comptes Rendus, li, 766–769.

(54.) *Lespiault.*—Observations faites à Briviesca (Vieille Castille) sur l'eclipse total de soleil du 18 Juillet, 1860. Comptes Rendus, li, 220–223. Institut, 1860, p. 259.

(55.) *Petit.*—Observations de l'eclipse du 18 Juillet faites à Briviesca. Comptes Rendus, li, 389–394; Cosmos, xvii, 152, 153; Institut, 1860, pp. 318, 319.

(56.) *Petit.*—Beobachtung der totalen Sonnenfinsterniss am 18 Juli, 1860. Astr. Nachr., liv, 75.

(57.) *D'Abbadie.*—Eclipse totale du 18 Juillet, 1860. Remarques de Mr. Faye. Comptes Rendus, li, 703–709; Institut, 1860, pp. 380–382; Cosmos, xvii, 583–585, 589–592; Astr. Nachr., liv, 277.

(58.) *Airy.*—On a result deduced by Mr. d'Abbadie from observations of the total eclipse of July 18, 1860. Monthly Notices, xxii, 3–5.

(59.) *Farnam, Maxwell, Lyte, and Micheliac.*—Observation de l'eclipse de soleil à l'hôtellerie sur le versant du sud du pic du Midi, Pyrenees. Comptes Rendus, li, 181, 182; Institut, 1860, pp. 389–399.

(60.) *A. Secchi.*—Observations faites pendant l'eclipse totale du 18 Juillet, 1860, au sommet du Mont St. Michel au Desierto de las Palmas en Espagna. Comptes Rendus, li, 152–162, 276–279, 386–388, 749–751; Institut, 1860, pp. 250, 251, 259, 260, 282–283; Cosmos, xvii, 151, 152, 242–329, 468–470; Heis, W. S., 1860, pp. 263, 264, 265, 366–368, 382–384; Astr. Nachr., liv, 35, 263.

(61.) *A. Secchi.*—Relazione delle osservazioni fatte in Spagna durante l'ecclisse totale del 18 Luglio, 1860, (Estratto.) Roma, 1860; printed from Cimento, xii, 147–180.

(62.) *A. Secchi.*—Aggiunta alla relazione delle osservazioni fatte in Spagna. Roma, 1860.

(63.) *A. Aguilar.*—Observation faite au Desierto de las Palmas de l'eclipse de soleil du 18 Juillet, 1860. Cosmos, xvii, 329, 330; Heis, W. S., 1861, pp. 5–7, 9–12, 17–18; Astr. Nachr. liv, 17.

(64.) *A. Aguilar.*—Communicacion del director del observatorio de Madrid al Comisario Regio del mismo, participándola los principales resultados obtenidos en la observacion del eclipse de sol del 18 de Julio, en el Desierto de las Palmas.

(65.) *A. Aguilar.*—Eclipse de sol del 18 de Julio, de 1860. Annuaire d. Observ. R. de Madrid, 1860, p. 171–257.

(66.) *Don Franc. de Paula Marquez.*—Memoria sobre el eclipse de sol de 18 de Julio, de 1860. Publicada de orden Superior. Madrid, 1861.

(67.) *E. Gautier.*—Observation de l'eclipse totale de soleil de 18 Juillet, 1860, à Tarazona, (Aragon,) Arch. d. Sc. Phys., (2,) ix, 236–247.

(68.) *A. Laussedat.*—Observation de l'eclipse du 18 Juillet, à Batna. Algérie. Comptes Rendus, li, 270, 271, 441–445; Institut, 1860, pp. 278, 322–324; Cosmos, xvii, 361, 362.

(69.) *Faye.*—Remarques sur l'hypothese de l'atmosphère de la lune à l'occasion de la lecture precedente. Comptes Rendus, li, 445–448; Cosmos, xvii, 362, 363; Institut, 1860, pp. 307–311.

(70.) *Mahmoud Bey.*—Rapport à son Altesse le viceroy d'Egypte sur l'eclipse totale du 18 Juillet, observé à Dongolah. Comptes Rendus, li, 680–684; Heis, W. S., 1860, 412, 413; Cosmos, xvii, 569–571; Institut, 1860, p. 374.

(71.) *Faye.*—Rapport sur l'observation de l'eclipse du 18 Juillet, faite en Nubie par Mahmoud Bey. Comptes Rendus, liii, 133–139.

(72.) *Alexander.*—On the results of the astronomical expedition to Labrador to view the eclipse. Report of American Association. Edinburgh J., (2,) xii, 295, 296.

(73.) *Gilliss.*—An account of the total eclipse of the sun, July 18, 1860, as observed near Steilacoom, Washington Territory.

(74.) *C. M. Goulier.*—Eclipse de soleil du 18 Juillet, 1860. Note accompagnant l'envoi de trois images photographiques faites à Metz par le capitaine de génie Lamey. Comptes Rendus, li, 148.

(75.) *Vernier, (fils.)*—Observations de temperature faites à Belfort durant l'eclipse, images photographiques de l'astre eclipsé. Comptes Rendus, li, 148, 149.

(76.) *W. de la Rue.*—The recent solar eclipse as seen in Spain. Illustrated London News, August 25, 1860; Athenæum, August 25, 1860; Heis, W. S., 1860; pp. 325–328, 329; Presse Scientifique, 1861, (3,) pp. 257–261.

(77.) *Lamont.*—Sur les protuberances rouges observées pendant l'eclipse de soleil du 18 Juillet, 1860. Bulletin de Bruxelles, (2,) x, 426–429; (Classe de Sciences, 1860, pp. 541–544.)

(78.) *Lamont.*—Die Sonnenfinsterniss vom 18 Juli, betreffend. Heis, W. S., 1860, pp. 308–310.

(79.) *C. von Wallenberg.*—Einige Mittheilungen über die totale Sonnenfinsterniss am 18 Juli, 1860, beobachtet zu Valencia. Astr. Nachr., liv, 65.

(80.) *Haase.*—Beobachtung der totalen Sonnenfinsterniss vom 18 Juli, 1860, zu Valencia. Astr. Nachr., liv, 337.

(81.) *Adolph.*—Beobachtung der partiellen Sonnenfinsterniss vom 18 Juli, 1860, zu Göttingen. Astr. Nachr., lv, 91.

(82.) *J. Spiller.*—Photographic observations of the solar eclipse, July 18, 1860. Phil. Mag., (4,) xx, 192–194.

(83.) *E. Quetelet, H. Hooreman.*—Note sur l'eclipse de soleil du 18 Juillet, 1860, observée à l'observatoire royal de Bruxelles. Bulletin de Bruxelles, (2,) x, 181–184, (Classe de Sciences, 1860, pp. 339–342;) Astr. Nachr., liv, 1.

(84.) *A. Quetelet.*—Eclipse partielle de soleil observée à Kensington le 18 Juillet, 1860. Bulletin de Bruxelles, (2,) x, 185, 186, (Classe de Sciences, 1860, pp. 343, 344; Institut, 1860, pp. 326–328.

(85.) *W. S. Jacob.*—Notes on the total eclipse of the sun of July 18, 1860, observed in Spain. Edinburgh Journal, (2,) xiii, 1–6.

(86.) *W. Ferrell.*—Narrative of the American expedition to N. W. British America, to observe the total solar eclipse of July 18, 1860. Silliman's Journal, (2,) xxxi, 139–142; Heis, W. S., 1861, pp. 36–39, 45–48, 51, 52.

(87.) *Zantesdeschi.*—Sur les phénomènes qui ont accompagné l'eclipse de soleil du 18 Juillet, 1860. Comptes Rendus, liii, 194, 195.

(88.) *Airy.*—Results of observations of the solar eclipse of July 18, 1860, made at the Royal Observatory, Greenwich. Monthly Notices, xxi, 155–157.

(89.) *Cantzler.*—Die Sonnenfinsterniss am 18 Juli, 1860, beobachtet zu Greifswalde. Heis, W. S., 1860, pp. 284, 285, 289, 290, 298, 317, 318, 329, 330 338, 339.

(90.) Accounts of the solar eclipse, July 18, 1860, as observed in England, at Greenwich Hospital, by Mr. Riddle; at Greenwich, by Rev. G. Fisher; at Maresfield, by Captain Noble; at Uckfield, by Mr. Leeson Prince; at Highbury, by Mr. T. W. Bure; at Haddenham, by Rev. W. R. Dawes. Monthly Notices, xxi, 16–27.

(91.) Correspondenznachrichten die Sonnenfinsterniss vom 18 Juli, 1860, betreffend. Heis, W. S., 1860, pp. 252, 253, 261, 262, 276, 277, 322–333, 389.

(92.) *G. Schweizer.*—Ueber die in der Nähe der Sonnenränder beobachteten Flecken vor und nach der totalen Sonnenfinsterniss des 18 Juli, 1860. Bulletin de Moscou, 1860, (2,) pp. 238–267.

(93.) Meteorologische Beobachtungen während der Sonnenfinsterniss vom 18 Juli, 1860, zu Bordeaux. Heis, W. S., 1860, p. 262.

(94.) *Krecke.*—Temperatur der Luft während der Sonnenfinsterniss am 18 Juli, 1860, zu Utrecht. Heis, W. S., 1860, pp. 343, 344.

(95.) *Baudrimont, Raulin, Houel, Royer et Micé.*—Eclipse solaire du 18 Juillet, 1860; observations de physique et de meteorologié faites à Bordeaux. Comptes Rendus, li, 145–147; Cosmos, xvii, 153, 154.

(96.) *L. Palmieri.*—Osservazioni meteorologiche e magnetiche durante l'eclisse ultima. Cimento, xii, 145–147.

(97.) *E. Desains.*—Observations thermometriques instituées pendant l'eclipse de soleil du 18 Juillet, 1860. Cosmos, xvii, 118, 119.

(98.) *Lorey.*—Sonnenfinsterniss am 18 Juli, 1860, beobachtet auf dem Paulsthurme, in Frankfurt am Main. Jahresbericht der Frankfurt Verein, 1859–'60, p. 53.

(99.) *Lindhagen.*—Jagttagelser öfver solförmörkelsen den 18 Juli, i Spanien. Öfvers. af Förhandl, 1860, pp. 383–404.

(100.) *A. Möller.*—Berättelse om en med auslag af almänne medel företragen resa föratti det une af Spanien, observera den totale solförmörkelsen af den 18 Juli, 1860. Öfvers. af Forhandl, 1860, pp. 405–414.

(101.) *A. Möller.*—Beobachtung der totalen Sonnenfinsterniss am 18 Juli, 1860, in Lund. Astr. Nachr., liv, 96.

(102.) *Bruhns.*—Beobachtung der totalen Sonnenfinsterniss vom 18 Juli, 1860, in Tarazona, in Spanien. Leipz. Ber., 1860, pp. 214–232; Archiv des Sciences Phys., (2,) xiii, 246–249; Zeitschrift für Naturw. xviii, 37–38.

(103.) *O. Struve.*—Bericht über die Beobachtung der totalen Sonnenfinsterniss vom 6, (18.) Juli, 1860, zu Pobes. Bulletin der St. Petersb., i, 385–396.

(104.) *Sabler.*—Beobachtung der partiellen Sonnenfinsterniss vom 18 Juli, 1860, in Wilna. Astr. Nachr., liv, 21.

(105.) *Von Littrow.*—Beobachtung der partiellen Sonnenfinsterniss vom 18 Juli, 1860, in Wien. Astr. Nachr., liv, 135.

(106.) *Th. Thiele.*—Solformörkelsen den 18 Juli, 1860, observeret i Vitoria. Nordisk. Univers., Tidskrift, 6 Aarg.. 11 Heft, 1860.

(107.) *D'Arrest.*—Beretning over Jagttagelsen af der totale Solformörkelse der indtraf i Spanien den 18 Juli, 1860, (preliminary notice.) Overs. over Forhandl, 1860, 195, 196.

(108.) *Bulard.*—Eclipse total de soleil du 18 Juillet, 1860, observée à Lambessa, (province de Constantine.) Comptes Rendus, liii, 509–512.

(109.) *Zantesdeschi.*—Intorno ai fenomeni osservati in Italia nel eclisse di sole, 18 Luglio, 1860. Cherbourg, 1861.

(110.) *E. Kayser.*—Beobachtung der Sonnenfinsterniss am 18 Juli, 1860, in Danzig. Astr. Nachr., liv, 225, 226.

(111.) *Legnazzi.*—Osservazioni del principio e della fine del l'eclisse del 18 Luglio, 1860, fatte all. I. R. Osservatorio Astronomico di Padova. Astr. Nachr., liv, 263.

(112.) *Maury.*—Eclipse of the sun, July, 1860, Washington. Astr. Nachr., liv, 11, 12.

(113.) *Wolf.*—Beobachtung der partiellen Sonnenfinsterniss zu Zurich. Astr. Nachr, lv, 337, 338.

In the great number of essays and notices contained in the above catalogues, space would not allow us to consider each one separately, even independently of the many repetitions which must thereby arise. I will, therefore, give a *general* account of the phenomena of the solar eclipse of the 18th of July, 1860, and therein, in order to simplify the citations as much as possible, will always adjoin, in parentheses, the *catalogue number* of the memoir to which reference is made in the statements.

The writings designated above refer both to stations where the sun appeared partially eclipsed, and also to those which lay within the zone of total eclipse. With reference to the first class, it will be sufficient merely to enumerate the names of the stations. They are as follows: Athens, (39;) Belfort, (76;) Bordeaux, (93, 95;) Breslau, (40;) Brussels, (83;) Dantzic, (110;) Frankfort-on-the-Main, (98;) Göttingen, (81;) Greenwich, (90;) Greifswalde, (89;) Haddenham, (90;) Highbury, (90;) Kensington, (84;) Kiel, (26;) Kremsmünster, (38;) Lund, (101;) Milan, (37;) Maresfield, (96;) Metz, (74;) Naples, (96;) Padua, (111;) Pic du Midi, Pyrenees, (59;) Rome, (61;) Storlus, (35;) Trieste, (36;) Uckfield, (90;) Utrecht, (94;) Vienna, (105;) Washington, (112;) Wilna, (104;) Woolwich, (82;) Zürich, (113.)

The observations made within the zone of totality are the only ones which are of especial interest; and in this respect there is in the above collection an important deficiency, since the observations recorded by the English astronomers in northern Spain have only been published as yet to a very limited extent and very incompletely.

The zone of total eclipse began in North America, traversed Spain from north to south, passed over thence to Algiers, and ended in the interior of Africa. In North America, the government of the United States sent two expeditions—the one, under the direction of Mr. Alexander, to the coast of Labrador; the other, in charge of Mr. Gilliss, to Steilacoom, Washington Territory; but observations could be made only at the latter point.

The best opportunity for observations was furnished in Spain, and thither, accordingly, most of the astronomers betook themselves.

Notwithstanding that from the first, by a circular sent from the directory of the observatory at Madrid to all European astronomers, and published in the Astronomische Nachrichten, lii, 253–256, as well as in the Monthly Notices, xx, 184–187, the endeavor was made to distribute the stations uniformly over the whole zone of total eclipse, this was but very imperfectly accomplished, and, instead of an equable distribution, there resulted a collection into three groups, namely:

Northern group, with Vittoria as the central point.—This group consisted of Messrs. Airy, O. Struve, W. de la Rue, Winnecke, Mädler, Prazmowski, Möller, d'Arrest, Weyer, Fearnley, Lindelöf, Lindhagen, Petit, d'Abbadie, Lespiault, Goldschmidt, Thiele, Burat.

Middle group; central point, Tarazona.—This includes Messrs. Le Verrier, Villarceau, Chacornac, Foucault, Ismail Effendi, Bruhns, Gautier, Novella.

Southern group; central point, Castellon de la Plana.—To this group belong Messrs. Secchi, Aguilar, Plantamour, Rümker, B. von Feilitzsch, Bremiker, Marquez, Carlini, Donati, Haase, Von Wallenberg, Ribeiro de Sousa Pinto, Ant. de Souza, J. C. de Brito Capello, Klinkerfues, Lamont.

The French government sent to Algiers, under Mr. Laussedat, a commission consisting of officers and professors of the Polytechnic School, who stationed themselves in Batua; and the Viceroy of Egypt sent the astronomer of Cairo, Mahmoud Bey, with a numerous retinue, to Dongolah, on the Nile, (19° 12′ 41″ north latitude.)

Let us first contemplate the progress of the phenomenon in general. A very important circumstance was noticed everywhere, viz., that when (observing with the glass screen) the sun seemed to have completely disappeared, and the screen was then quickly removed, a bright solar crescent was still visible, disappearing some twenty or thirty seconds later. This is the same phenomenon which Mr. Airy first saw in 1842, at the Säperga near Turin, and described by saying that he had observed the sun vanish *twice* behind the moon. In the present case, some observers state that they saw a second solar crescent; others only remark that, after removing the screen, there was a dazzling brilliancy which compelled them to withdraw the eye from the eye-piece. This circumstance is especially important for the reason that it has influence upon the observed duration of totality, since it is evident that this duration will come out longer or shorter, according as the beginning and end of totality are observed with or without the screen; also, the intensity of shade of the screen will have its influence. The greater part of the observers probably observed the beginning with and the end without the screen.

During the second vanishing of the sun, or even a few seconds earlier, numerous intensely red rays issued from the moon's limb, the smaller ones of which soon disappeared, but the larger showed as *protuberances* after the eclipse was completed. According to some observers the vanishing solar crescent transformed itself into an intensely red border; while others saw, at the moment of the sun's vanishing, the whole moon surrounded by a small red border, either of red pearls or flames, which seemed to run around it. It was noticed by every one that a red border preceded the appearance of the sun on the west side.

The protuberances appeared upon the east, south, and north sides almost simultaneously, but only towards the middle of the totality did they come out upon the west, and gradually increased in height, while the eastern ones continually diminished, and entirely disappeared. Their color was red, more or less intense, and here and there orange. The protuberances were better seen with a light red glass screen than without any screen at all, and with such a glass could be longer followed even after the totality, a circumstance of which great advantage can be taken in future observations.

No ground was given for the assumption of a connexion of the *protuberances* with the *solar spots*.

In the *corona* there were to be distinguished the *innermost small ring*, the *outer broad ring*, and the *rays* or *halo*.

Of the innermost small ring the moon's limb formed the interior limit, and a sharp circular line concentric with the moon's limb, and about two minutes distant from it, the outer limit. The light was silver white, and of equal intensity throughout, or perhaps a little fainter just at the moon's limb.

The outer ring diminished in intensity as the distance from the moon's limb increased, and an exterior limit could not be assigned to it.

The rays reached a distance of more than a diameter of the moon, and were partly straight, partly curved.

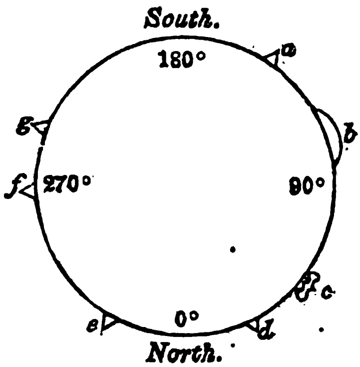
The corona was seen for several minutes before and after the totality.

Phenomena exactly corresponding to the fringes and pearls described by Baily were not recognized.

The darkness during totality was, in America, equal to that of night. In Spain and Algiers there remained a twilight sufficient to enable the observer, without a lantern, to recognize the seconds of the chronometer, and to read coarse print. The planets and stars of the first magnitude in the vicinity of the sun were easily perceived. As regards the planet of Lescarbault, its non-appearance contributed to confirm the opinion which the great number of astronomers had already formed relative to it.

The dark spots or fringes which were first seen, in 1842, to pass over on the floor or on white walls immediately before the totality attracted the attention of

many observers in Spain and Algiers, who were occupied with the general progress of the phenomenon; and it appeared that they begin about a minute before the vanishing of the sun, and move on parallel with the solar crescent, *i. e.*, parallel with the tangent of the point of the moon's limb, where the sun vanishes.



A collation of the observations decides at once one of the most important points of dispute, inasmuch as it comes out decidedly that everywhere the principal protuberances appeared at the *same* points of the moon's limb.

The most conspicuous protuberances are designated in the adjoining figure. The first, which appeared immediately after the sun vanished, was *a*, whose angle of position (taken in some cases only from drawings) is thus given:

155°	Bruhns, (Tarazona.)	155°	W. de la Rue, (Rivabellosa.)
154	Secchi, (Desierto.)	154	Winnecke, (Pobes.)
155	Aguilar, (Desierto.)	144	Novella, (Tarazona.)
143	Plantamour, (Castellon.)	156	d'Abbadie, (Briviesca.)
160	Lamont, (Castellon.)	148	Thiele, (Vitoria.)
140	Von Feilitsch, (Castellon.)	155	Goldschmidt, (Vitoria.)

The long mountain ridge *b* is also to be perceived in all the drawings. The angles of position are, however, less accurate on account of its extent.

The protuberance *c* is especially deserving of notice, because it appeared separated from the moon's limb. Its position was:

55°	Airy.	45°	Plantamour.
60	Bruhns.	25	Novella.
59	Secchi.	57	W. de la Rue.
59	Aguilar.	63–78	Winnecke.
		58	Goldschmidt.

The protuberance *d* appeared under the position-angle:

28°	Aguilar.	35°	Bruhns.
16	Struve.	30	Lamont.
36	Winnecke.	25	Goldschmidt.
		22	Airy.

The protuberance *e* was noticed by only a few observers, and seems afterwards to have formed part of an extended mountain ridge. The position was given as follows:

328°	Novella.	330°	Aguilar.
320	Struve.	340	Bruhns.
		350	Goldschmidt.

The protuberance *f* was, at some places, observed as standing by itself; at others it formed only a part of a long mountain ridge. The position-angle was:

260°	Bruhns.	265°	W. de la Rue.
277	Secchi.	260–263	d'Abbadie.
270	Plantamour.	265	Aguilar.
270	Lamont.		

The position of protuberance *g* was given as follows:

255°	Thiele.	235°	Aguilar.

It is impossible to establish a complete accordance between the observations of the different astronomers, both because no observer has noted *all* the protuberances, and also because, in estimating or graphically representing the position-angles, the accidental errors may come out quite large, as is already evident from the examples cited. To this may yet be added that on the west side of the moon, at the middle of totality, only *single* protuberances appeared; while later, on this side, they extended to long mountain ridges, so as to present a different aspect every moment, rendering an identical reference impossible.

The attempts made in Desierto de las Palmas and Rivabellosa to photograph the phenomena of the eclipse led to the satisfactory result that not only some success is to be obtained, (which, indeed, could scarcely be doubted after the attempts made in Königsberg in 1851,) but also that the phenomena are much more correctly and completely recorded than by direct observation. The position-angles of the protuberances obtained by photographing (very uncertainly, indeed, on account of the smallness and want of precision in the images) were given by Mr. Aguilar as follows:

	Desierto de las Palmas.	Rivabellosa.
1. Protuberance *d*	22°	28°
2. ....do..... *c*	57	57
3. ....do..... *a*	159	154
4. ....do	194	197
5. ....do	231	230
6. ....do..... *g*	260	265
7. ....do..... *f*	276	278
8. ....do..... *e*	340	346

The fourth protuberance was observed by Mr. Secchi (pos. 195°) and Mr. Aguilar, (pos. 193°;) the fifth by Mr. Secchi, (pos. 231°.)* Also, the long mountain ridge *b* occurs in the photographs. Moreover, the photographs show a considerable number of protuberances not included in the preceding list, and among these even very prominent ones, *of which no trace was to be perceived by direct observation.* The explanation of this fact presents many difficulties, since, if we say that the light of those protuberances may act chemically without affecting the retina of the eye, we must not forget that in practice hitherto no example of this sort has yet been exhibited.

The fact that the photographs obtained in Metz by Mr. Goulier, (74,) and sent to the Paris Academy, show a sort of corona close to the solar crescent, which could not be seen by direct observation, appears to be attributable to accidental causes, and certainly should not be considered analogous to the above-mentioned phenomenon.

If we would consider more particularly the questions to be brought to test by the solar eclipse of the 18th of July, 1860, we find in the first rank those relating to the *nature of the protuberances.* The manifold investigations to which the earlier eclipses gave rise were so far from bringing a definite opinion with general acceptance that, even now, those who explained the protuberances as phenomena of interference or inflexion, and those who considered them as solar clouds, were about equally divided. In the above-mentioned preparatory memoirs both hypotheses are defended; and, in fact, Messrs. Airy (5) and Von

* Singularly, both these protuberances are wanting in the drawing made by Mr. Aguilar after the direct observation.

Littrow (12) have pronounced very decidedly in favor of solar clouds, and Mr. Von Feilitzsch (22) for phenomena of interference. The detailed theoretical references given by the latter deserve very special attention, and include all the optical phenomena of total eclipses—the corona, the rays in the corona, and the protuberances. For producing the latter there are assumed at the moon's limb isolated conical elevations of about 500 feet altitude and base, which are yet so small that they could not be seen with telescopes magnifying 300 times; and it is also shown that the same theoretical development explains the origin of an *isolated* protuberance, if we assume at the moon's limb a very high isolated mountain summit. With regard to the hypothesis advanced by myself, (2,) that the colors of the protuberances are produced by inflexion of light at the moon's limb, but their form by small masses of vapor floating in our atmosphere, this has been set aside by the circumstance mentioned above, that the same protuberances were seen at different places. Nevertheless, I cannot yet entirely give up the opinion that the vapors of an atmosphere—that is, the condensations caused by reduction of temperature next to the inmost shadow—do exercise a very considerable influence upon the phenomena of total eclipses, and especially upon the forms of the protuberances.

If we examine how the opinions relative to these questions stand now *after* the observations of the total solar eclipse of 1860, we find the vote comes out nearly as follows:

In favor of solar clouds, more or less decidedly, are—Messrs. Airy, Le Verrier, Secchi, Aguilar, Struve, Mädler, Gautier, Bremiker, Gilliss, Winnecke, Petit, Prazmowski, Lespiault.

In favor of interference phenomena are—Messrs. Plantamour, d'Abbadie, Marquez, Legrand, Faye, Lamont.

A preponderating number have, therefore, declared themselves in favor of the first opinion. Hereby we must not omit to consider that every one who desires to combine different observations of a solar eclipse into a single result is compelled, at the same time, to *interpret critically* and to *supply deficiencies.* In the shortness of time and the incompleteness of the apparatus no observer can completely and accurately take in the whole phenomenon, and therefore an interpretation and completion seems necessary and justified. But thereby the deciding ground becomes so far doubtful, that probably the greater part of the non-participating astronomers will consider the case as not yet ripe for decision.

The criteria according to which we must decide are very simple. Supposing that the moon moves over the sun exactly from west to east, then the protuberances, whether solar clouds or caused by interference, will first appear in the east, and gradually diminish in size, will come out later in the west and increase in size; while upon the north and south the magnitude must remain unvaried. If the protuberances are solar clouds, there are yet to be added the special conditions:

1. That the diminution of altitude in the east and the increase in the west must exactly correspond to the relative motion of the moon.

2. That the protuberances must remain unchanged in form and color.

3. That with the northern and southern protuberances there must be changes of the angles of position corresponding with the relative motion of the moon.

Applying these propositions to the several protuberances mentioned above, it appears that with *d* the altitude should have remained unchanged; but the position-angle should in each minute have diminished about 1.°9. That, furthermore, the protuberance *c* should have diminished by 14″, and *a* by 23″ in each minute, and the protuberance *g* should have increased 24″, *f* 26″, and *e* 20″ in the same time.

Direct measures, with reference to the given criterions, were made by Messrs. Airy (88) and d'Abbadie, (57,) and the former found for the position-angles

of *d* and *c* in equal intervals of time the following values, which, however, represent neither an increasing nor a diminishing series, namely:

d		*c*
25° 50′		55° 50′
20 20		56 20
	(Two observations lost.)	
20 20		56 20
23 20		53 20

while the latter measured three altitudes of the protuberance *a*, of which the first is doubtful, and, as Airy has circumstantially shown, (58,) if the correction required by the observer himself be adopted, would be *against* the hypothesis of solar clouds; but if the correction be not adopted, would be *in favor of* that hypothesis. By indirect methods, comparing with the position of the solar crescent, Bruhns (31) (102) obtained two position-angles of the protuberance *d*, which he was able to follow from 2 minutes before until 8 minutes after the total eclipse, and found that in an interval of 13.7 minutes the angle of position had diminished 26.°3, an argument of weight in favor of the assumption of solar clouds. On the other hand, Von Feilitzsch (44) (45) determined by measurement the diminution of the protuberance *a* in one minute to be 45″, and Plantamour (47) found it more than 30″, while it should have amounted to only 23″. The latter pointed out also that the floating cloud *c* vanished before it could be reached by the advancing moon's limb; and entirely similar results, pronouncing decidedly against the assumption of solar clouds, were obtained by Mr. Thiele, (106,) who compared the altitudes of the protuberances measured at definite instants with the times of their disappearance, and thence computed the diminution of altitude.

Besides these measurements, there are in the material before us no other *numbers* which could lead to a decision. The progress of the eclipse, however, produced in many observers, among them Messrs. Plantamour, (47,) d'Arrest, (107,) Legrand, (46,) Goldschmidt, (41,) (42,) &c., the definite impression that the changes of altitude did not proceed with uniform velocity, and my own perceptions agree with this. On the other hand, Mr. Secchi (60) (61) brings up the circumstance that the colors of the protuberances were very different from the interference colors exhibited in optical experiments.

Since the central shadow passed over the whole distance from the northern to the southern coast of Spain in ten minutes, the protuberances considered as solar clouds should have appeared at all the stations the same in form and colors. Now, upon comparing together the drawings and descriptions of the different observers, it will be always possible, by interpreting and completing deficiencies with considerable freedom, to produce a similarity; but without such interpretation and completing, there is certainly to be found no satisfactory agreement at all.

Definite resting points might be gained by comparing the forms which the same observer saw at different moments during the totality; and yet, in this respect, we find contradictory testimony, for, while Bremiker (30) could perceive no changes, Messrs. Plantamour, (47,) Von Feilitzsch, (45,) Bruhns, (31,) and Goldschmidt, (41,) (42,) did observe changes of form and color in several protuberances. I noticed the same thing, with all certainty, in the protuberance *a*.

With regard to the corona, and the rays therein contained, the observers appear generally to have attained to the conviction that they do not belong to the sun, but are occasioned by interference at the moon's limb, and partly also by the vapors in our atmosphere.

The question whether the same rays were seen at the different stations cannot be decided with definiteness, for, while in the drawings at Pobes and Tarazona

a great resemblance can be perceived, the other representations differ so widely from each other that doubts must arise respecting the identity of the objects.

I now pass to the special contents of the individual memoirs, but limit myself to mentioning that which is worthy of especial notice, or is strikingly discrepant.

The most comprehensive and important among the above-mentioned memoirs is that of Mr. Aguilar, (65,) in which we find, not only a review of all the conclusions arrived at by Spanish observers, but also a collection of many results from foreign observers. After a historical introduction we find the limits of totality determined from the data given by professors, officers, and engineers, who, partly voluntarily and partly commissioned by their governments, had stationed themselves at corresponding points. It results therefrom that the zone of totality agreed in diameter with the prediction, but, in position, must be carried somewhat N.NE. of the predicted place. Next follows an investigation of the duration of totality, which was everywhere found to be shorter than predicted. In fact, the correction amounted to—

15″ in Vitoria.	17″ in Moncayo.
16 in Briviesca.	16 in Castellon.
16 in Herramelluri.	15 in Desierto.
16 in Burgos.	12 in Campvey.

That the computed duration should require a correction, while the computed breadth required none, is a contradiction, which Mr. Aguilar explains by the remark that the correction corresponding to the above numbers would amount to only 600 meters, and we can only decide about so small a quantity when a complete collection of the observed data is before us.

If it should finally appear that there was a diminution of the breadth of zone corresponding to the diminution of duration of totality, then Mr. Aguilar thinks it probable that, in accordance with the idea of Faye, (69,) to be exhibited below, we must assume a lunar atmosphere. That the phenomenon may be ascribed to an entirely different cause has been already indicated above.

Further on Mr. Aguilar mentions the different views relative to the corona, such as the questions whether it is single or double; whether it extends out further at the sun's equator than at the poles; whether its light is polarized or not. In respect to the latter question the observations of Messrs. Secchi, Barreda, Rodriguez, and, above all, of Mr. Prazmowski, have decided that it is to be answered affirmatively, assuming thereby that the polarizing reflection takes place in the atmosphere of the sun, and not at the moon's limb, or in the atmosphere of the earth.

The question of the protuberances is treated most at length. Mr. Aguilar brings up the facts noted by the Spanish observers at Bilbao, Vitoria, Tudela, Logroño, Casarejos, Lortora, and Ibiza, which he considers accordant with the idea of solar clouds, then expresses his doubts relative to the observations of Gijon and Oviedo, which do not harmonize therewith, and states, in special detail, the things noticed by himself and some foreign astronomers. In this connexion steel-engraved copies of four photograms, obtained at Desierto de las Palmas by Mr. Monserrat, by help of an apparatus belonging to Mr. Secchi, are added by way of elucidation.

Finally, the last chapters relate to the intensity of the solar light, the effect upon plants, meteorological determinations, and effect upon animals. An appendix gives a summary view of all the stations and observers on the line of totality from the Bay of Biscay to the Mediterranean.

The memoir of Mr. Marquez (66) is very thorough, and of great interest. It contains, first, the principal moments of eclipse, the description of its progress, the position and magnitude of the protuberances, represented by a sketch* drawn

*I assume that No. 7, in the drawing of Mr. Marquez, is identical with *a*; Nos. 1, 2, 3, with the mountain chain *b*; No. 8 with *d*; No. 9 with *c*; and No. 11 with *f*. Over the protuberance *a* Mr. Marquez noticed two isolated points.

by eye, and differing considerably from the data of other observers, after which follow very complete meteorologic, magnetic, and photometric observations. It is worthy of notice that an inner ring was not seen in the corona; on the other hand the whole circumference of the moon appeared surrounded by a red border. It is stated, moreover, that the protuberance *f* was seen proceeding out from the moon's limb before the solar crescent; and, finally, we must also mention the noted peculiarity that immediately before the vanishing and after the re-appearing of the sun, black, mountain-like elevations of the moon's limb (somewhat similar to the phenomenon described by Baily) projected themselves upon the various solar crescent. The greater part of the memoir is taken up with the collation of previously observed phenomena of total eclipses, and the criticism of the theories formed for their explanation, wherein the author expresses, as the final result, his very decided opinion that we can only assume inflexion, or interference of light, at the moon's limb.

The first memoir of Mr. Airy (49) is to be considered as only a preliminary account, as all the observations made by those who participated in the British expedition are to be collected together in a large volume and published at the expense of the British government. In the second memoir (88) the corrections of the solar tables of Le Verrier, and lunar tables of Hansen, are deduced from the observations made during the eclipse with the great equatorial of the Greenwich Observatory, and the results are as follows:

	Correction.
Diff. AR ☾ — AR ☉	= —1.″1
Diff. Decl. ☾ — Decl. ☉	= —4. 0
Sun's diameter	= +0. 3
Moon's diameter	= —2. 4

The memoir of Mr. Bremiker (30) contains, in addition to the determination of time and the principal instants of eclipse, also the position-angles of the protuberances, and some data respecting their form. He did not observe any changes of form, and, according to the whole course of the phenomenon, he explains the protuberances as solar clouds. It is worthy of notice that the floating cloud *c* was not perceived either by him or myself, (the distance between us was only a few steps,) while Mr. Plantamour, whose station was some hundred feet further west, saw it distinctly. We meet a similar paradox also at Desierto de la Palmas (65) and Oropesa, (66.) Mr. Bremiker appends to his memoir a brief investigation respecting the brilliancy of Venus, which, at the time of totality, he estimated to have one and one-half time the brightness of Jupiter, while, according to Lambert's formula, it should have given much less light. He shows that the observation may be satisfied by assuming that the *atmosphere of Venus* also reflects light, so that the formula of brilliancy must consist of two terms, whose coefficients he determines.

Mr. Plantamour (47) brings up, in his brief but very precisely written exhibition of the course of the eclipse, various facts which contradict the assumption of solar clouds, and gives three drawings, representing the beginning, middle, and end of totality, in which are found the protuberances *a, b, c, f*, and a mountain-chain which covers the whole distance between *e* and *f*.

In the second paper (48) he endeavors to defend his drawings, and the statement that the protuberance *e* vanished without coming into contact with the moon's limb, against the objections of Mr. Secchi, (62.)

Mr. Gautier, (67,) without having perceived anything peculiar or different from other observers, pronounces with great decidedness against the hypothesis advocated by Mr. Plantamour, and appears to assume that the sun is surrounded by a cohering red cloud-stratum with steep elevations and depressions. In the drawings given by him we notice the protuberances *a, b, c, e, f*, and a long mountain-chain between *e* and *f*.

Mr. Goulier (74) brings out in a short notice the circumstance that, in the photographs obtained by Mr. Lamey in Metz, the solar crescent appears surrounded on all sides by a bright light, of which the direct observations have shown no trace.

The results of Mr. Chacornac, (24,) as well as the apparatus used by him, are materially different from the rest. The telescope employed, made by Foucault, had a silvered mirror of 0.4 metre (15 Paris inches) [aperture,] and was mounted equatorially. The investigation related exclusively to the protuberance *d*, whose position he gives as 50° (30°?) eastward from the north point. While other observers compared the protuberance to mountain-tops, mountain-chains, or to clouds, Mr. Chacornac declares this comparison wholly inaccurate, and finds in the appearance great similarity to numerous gas-flames, or, better yet, to a burning pile of straw, or of loose combustible material, on which a current of air is acting in such a manner as to bend the many flames into different directions. The protuberance consisted of two separate parts: a larger part, where it seemed as though the burning had just commenced; and a smaller, where apparently the fire had already penetrated through the material, and the burning was quietly going on. From the circumstance that some parts appeared very distinct, while others seemed to be in a manner wrapped in cloud, we should infer, says Mr. Chacornac, that some were nearer, and others at a greater distance, an idea which Mr. Secchi has also brought forward. Although Mr. Chacornac directed his especial attention to only one point, he nevertheless swept repeatedly with his telescope over the whole circumference of the moon, and so had opportunity to convince himself that all the protuberances presented a similar aspect. It is known that Mr. Arago considered the luminous envelope of the sun as burning gas, and Mr. Chacornac seems to have had this idea in his mind while describing the protuberances.

The different memoirs of Mr. Secchi (60) (61) (62) are of especial interest, partly on account of the observations which he himself made, and partly by reason of the connexion into which he has brought his own observations with those of others. He considers the protuberances as portions of the luminous envelope of clouds by which the sun is surrounded, and holds accordingly that the solar atmosphere is less extended in the polar regions than toward the equator, and that also the agitation of the atmosphere is less at the poles. His remark (not fully carried out in all respects) that the photographs obtained by himself and Mr. de la Rue are identical is especially noteworthy, as also his explanation of the circumstance that protuberances appear in the photographs which could not be perceived by direct observation with the telescope. The changes of solar heat during the progress of the eclipse were determined by Mr. Secchi by means of a thermo-multiplier of Melloni; also magnetic and meteorologic observations were noted down.

Mr. Prazmowski (51) gave himself to the problem of investigating the polarization of the corona and of the protuberances, for which purpose he had constructed two different instruments. The first, consisting of a telescope magnifying 22 times, with a quartz plate in the focus, and a Nicol's prism between the first and second eye-lens, showed a *strong* polarization of the light of the corona in which the polarizing plane was perpendicular to the moon's limb—a result which entirely agrees with previous determinations, and with the above-mentioned observation of Mr. Secchi. The second instrument, a telescope of the same kind as the preceding, but with double the power, had a scale of quartz between the first and second lenses, and before the eye-piece a double-refracting prism, with a *small* refracting angle, so that the two images of a protuberance appeared near together, (the distance was only 1½ minute,) while the two images of the corona projected themselves upon each other, and formed a white ground. In this way it became possible to decide the hitherto unsettled question respecting the polarization of the protuberances by ascertaining that their light is not

polarized. "Is it allowable," asks now Mr. Prazmowski, "to conclude from this that the protuberances are solar clouds which consist, not of gaseous, but of vapory or fixed particles?"

Among the facts noted by Mr. Lespiault (54) we may point out this, that rays belonging to the corona proceeded out from very many points of the moon's limb, but irregularly in direction and distribution, and some were also curved near the outer limit of the corona. The irregularity showed itself very manifestly at about 233° from the north point, where the rays seemed to cross in all directions. The largest ray in the corona was from 80° to 110° distant from the north point. He measured the altitudes and bases of three of the protuberances, *a*, (?) *e*, (?) *d*, but without giving the time.

The communication of Mr. Bianchi (43) respecting the identity of the protuberances of 1842 and 1860 would be of greater weight if more particular references were added. This important defect, and then the objections that must arise in consequence of the different relative position of sun and moon, and the circumstance that Mr. Bianchi does not seem to have occupied himself specially in astronomical works, gives but little hope that his propositions would be established. The *approximate* agreement which we perceive in the drawings of different eclipses relative to the position of single protuberances loses much in weight when we consider the great number of the protuberances.

Mr. Faye (50) did not observe the eclipse himself, but only collected observations and compared them with earlier statements, and has endeavored to show, in opposition to the opinion of his colleague, Mr. Le Verrier, (23,) that the hypothesis of solar clouds is untenable, partly by reason of the difference of form seen at different localities, partly by reason of the rapid changes of form and color which are manifested during the totality, and partly on account of the impossibility of referring the phenomena of different eclipses back to a common fundamental point. Thus we have observed white protuberances, rose-colored protuberances, intense red protuberances, red and orange protuberances, peach-red protuberances, violet protuberances, black protuberances, white protuberances, with black edges, without any reason having been assigned for these colors, and the transition from one to another. Mr. Faye then speaks of the phenomena of the corona and the halo of rays connected with it, which, according to his remarks, cannot be considered as belonging to the sun; and, furthermore, he does not acknowledge as correct the conclusions drawn from the polarization phenomena.

Mr. Petit, (55,) (56,) who made numerous measurements of the heights of the protuberances, (not given, however, in his memoir,) considers the hypothesis of solar clouds as completely established by the whole series of recent observations, and remarks, at the same time, that not the least ground is given for the assumption of identity of the protuberances of 1842 and 1860. In the corona, which he saw 12^m before and $2^m\ 46^s$ after the totality, he distinguishes three concentric rings—an innermost brilliant ring of $7'\ 30''$ breadth; a second ring, $9'\ 30''$ in breadth; and an outer ring, $28'$ broad, consisting of less regular light. Barometer and thermometer observations are also added.

The expedition sent to Algeria, under direction of Mr. Laussedat, (68,) constructed a temporary place of observation before the gate of Lambersa, and obtained, during the eclipse, various results which were transmitted to the Paris Academy at the same time with the very general report lying before us. The results communicated called up an academic discussion, in which Mr. Faye (69) remarked, that since, according to Hansen's statement, there is an atmosphere on the side of the moon opposite to the earth, and, according to Herschel, the temperature of the moon's surface in consequence of the long-continued sunshine reaches at least to the boiling point of water, the lunar atmosphere at the time of new moon must, by reason of the expansion, spread out, and *become visible at the sides of the moon.* He shows how in this way many phenomena of the

total eclipse of 1860, namely, the shortening of duration of totality, the visibility of the moon's limb before and after totality, &c., may be explained.

Mr. d'Abbadie (57) observed position-angles and altitudes of the protuberances *a* and *e*, and, in fact, the latter was observed before the appearance of the sun with the position-angle of 260°, and after the appearance of the sun, as a new protuberance, with the angle of 263°. The conclusions to which his observations lead have been already mentioned above. His polarization observations agree, indeed, with those of Mr. Prazmowski, but cannot be considered as decisive.

The account of Mr. Gilliss (73) is very remarkable, and we can only wish that the things noted could have been exhibited in more detail and explained by drawings. The station was in a prairie (Muck Prairie) near Steilacoom, in a bleak and little cultivated part of Washington Territory, and the dampness was so great that the object-glass of the telescope required to be wiped off from time to time, as a deposition was constantly forming. If we assume that in the photographed drawings, accompanying the memoir, north and south only are inverted, and not east and west, so that south is above, north below, west on the right, and east on the left, then Mr. Gilliss observed protuberance *g* with the position-angle of 255° to 258°, and this came out *first*, and with striking brightness as a cloud-pyramid of 2′ base and 1′ altitude. As the moon advanced the base increased, while the altitude remained the same; notwithstanding, the appearance made an impression upon Mr. Gilliss as though the protuberance came gradually more into view behind the advancing moon. A smaller protuberance (doubtless *f*) appeared simultaneously under the angle of 268° to 273°, and towards the end of totality the protuberance *b* (?) was also perceived. These are the only objects which Mr. Gilliss *specially* mentions. He remarks, however, that the number of the protuberances was considerable, and that they commenced to appear about 30^s after the beginning of totality, after a small white line had been seen immediately around the moon's limb, and outside of this line a crown of red points or pearls which seemed to run around the moon. But the most striking thing in the appearance were rainbow-like and rainbow-colored small bands of equal radius with the moon, which, in great number, following each other upon the dark lunar disk, moved inward toward the centre from east and west. Mr. Gilliss leaves it undecided whether a real appearance was seen here, or only an optical phenomenon arising from physiological causes, yet he adds a short description by Mr. Goldsborough, at Steilacoom, from which he thinks it may be concluded that the latter saw the same phenomenon. At the beginning of the totality the moon showed itself *spherical*, as though seen in a stereoscope.

Mr. Burat (25) designates the outer limit of the corona as elliptical in such a way that the breadth at the solar equator was greater, and less at the poles. Among the protuberances he noticed *b, c, d, e*, but no accurate comparison can be instituted, as he has not given the times.

Mahmoud Bey (70) observed the eclipse in Dongola, on the Nile, and saw at first 6, but near the end of totality 7 protuberances, among which were *b*, (observed position-angle 109° to 121°,) *f*, (observed angle 278°,) and *c*, which last appeared as consisting of two isolated clouds.

In the memoir of Mr. Mädler (34) it is especially worth while to notice the indication of a circumstance, not previously brought into consideration, by which a decisive confirmation or contradiction of the optical hypothesis is rendered possible. For since, under the conditions which obtain in solar eclipses, the moon's poles can have no libration, but the effect of libration at the east and west limbs is included within quite narrow limits, therefore the same protuberances must always appear at the poles in case they are caused by elevations at the moon's limb; and, as regards the east and west limb, there will be, at least in the course of a long period of time, total eclipses with *the same* libration, when

also the same protuberances should then appear at the sides of the moon. From the further proposition of Mr. Mädler, to use the ten-year period of solar spots in a similar manner, and compare the total eclipses which occur at equal phases of this period in order to decide whether the solar spots have a connexion with the protuberances, but little success may be anticipated generally. Very instructive lithographic plates are appended to the memoir, where we find all the hitherto observed protuberances represented. Mr. Mädler himself, in Vitoria, noted the protuberances *a*, *b*, *d*, *e*, and two smaller prominences besides; the observation of these, and comparison with the statements of other observers, leads him to the conclusion that solar clouds, and not diffraction or inflexion, are the cause of the phenomenon.

Mr. Thiele (106) gives a sketch of the protuberances, together with an estimate of the altitudes and position-angles; whence we can deduce that he saw the protuberance *a*, (pos. 148°; initial altitude, 2′; vanished one minute forty-six seconds after beginning of totality,) the mountain chain *b*, (pos. 90° to 120°,) the floating cloud *c*, (pos. 46°,) the protuberance *d*, (pos. 28°,) and the protuberance *e*, (pos. 345°.) From his own observations and those of others he deduces the velocity with which the moon apparently advanced over the protuberances, and finds the numerical result two or three times as great as it should have been upon the supposition that the protuberances belonged to the sun.

Mr. Von Wallenberg (79) observed very near the limit of the zone of totality in Valencia, and appears to have seen the protuberances *f* and *g* at the lower, and then *a* and *b* at the eastern limb of the moon. He describes the rays of the corona as uneven, and with cloud-like termination, and notes three in particular, one of which (slightly curved to the south) seemed to proceed from between the two eastern protuberances, and the other two (hook-shaped, with their concave sides towards each other) to proceed from the vicinity of the two lower protuberances. It may also be added as worthy of remark, that, at beginning of totality, the narrow solar crescent did not run together at the middle, but towards a small notch in the moon's limb somewhat on one side from the middle; and here a point of light remained behind, and vanished 15 seconds after the crescent.

Mr. Goldschmidt (41) (42) observed the protuberances *a*, *b*, *c*, *d*, (whose position-angles were probably given not from his own observation, but from the photographic determinations of Mr. Secchi,) and another protuberance at 195°, and two small ones at 36° and 60°. From his circumstantial description we perceive that before the vanishing of the sun he saw a gray-cloud stratum, situated at the sun's limb, just where the protuberance *b* appeared afterwards; that the protuberance *e* in the course of the totality changed considerably in form and color, and that *d* remained visible yet 4 minutes 40 seconds after the reappearance of the sun. He ascribes to the corona a yellow color; he compares the protuberances, whose altitudes he gives about twice as great as other observers, to glowing wood coals. He brings up as a thing especially noted that, during the totality, "the dark moon had had an inner broad and defined limb."

Mr. W. de la Rue (76) describes, first, the preparations which he had made for photographing, and then informs of the result, which consisted in obtaining two photographs during the totality, and thirty-one during the rest of the course of the eclipse. He, himself, observed the phenomenon with a telescope, in whose focus was applied a glass with lines for helping to estimate magnitude and position of protuberances; and he saw some minutes before totality, when he had diminished the light by reflection from a glass surface, the whole circumference of the moon and a bright protuberance eastward from the zenith. Afterwards, immediately before the sun vanished, he could, without diminishing the light, discern the floating cloud *c*, and a whole series of protuberances

further to the east. He adduces, as worthy of remark, that in the position-angle 72° a large protuberance appeared in the photographing, of which he had seen no trace in the direct observation, although the region was completely swept over by him.

With regard to my own observations, (77,) (78,) of which the results will not be published until a later day, I remark, that I saw only the protuberances *a*, *b*, *d*, and *f*, at the place of the floating cloud. I noticed rays belonging to the corona which were not perpendicular to the moon's limb, but were inclined southward.

The description given by Mr. Mannheim (28) of the movable fringes forms a part of the general report made by the commission sent to Algeria by the Polytechnic School in Paris. We see therein that the fringes were rectilinear and entirely colorless, and at first following each other at distances of one decimeter, and afterwards at smaller distances and with greater rapidity. In this connexion a quotation is introduced from the report of Arago upon the solar eclipse of 1842, wherein the explanation is pronounced difficult and uncertain.

Mr. Jacob (85) belonged to the British expedition which went to Spain under the direction of Mr. Airy for the purpose of observing the eclipse, and chose its station in the Pass of Peñacenada, between Vitoria and Logroño. From the preliminary notice which he communicates respecting the protuberances, we deduce that he saw the protuberance *a*, the mountain-chain *b*, the floating cloud *c*, and the protuberance *e* at precisely the same points of the moon's limb at which they appeared in southern Spain. With reference to the protuberance *e*, it is remarked that it first appeared shortly before the end of totality.

I believe that in the preceding pages I have brought into notice the most important points from the extremely comprehensive material before us. If I have not more closely considered various classes of observations relating to special questions, such as magnetic, meteorologic, photometric observations, or observations of colors and lines of the prismatic spectrum, the reason is, that as yet no noteworthy results have been deduced from those observations, and partly, also, because the questions in view, as of the absence of an influence of the eclipse upon the barometer and the magnetic needle, might be considered as decided by previous investigations.

In relation to the expeditions undertaken into Spain, I only add yet the remark, that they experienced on the part of the inhabitants the most friendly reception, and on the part of the authorities all possible support and furtherance in carrying out their scientific labors; and all those who participated in the expedition, without exception, have in the warmest terms expressed their acknowledgments.

ECLIPSE OF THE SUN, APRIL 25, 1865.

PARIS, *June* 23, 1865.

SIR: I have the honor to address to you the copy of a very interesting letter which I have received from a distinguished savant, M. le Baron de Prados, of Rio de Janeiro, on the total eclipse of the sun of the 25th of April last.

It appears to me important that this letter should be published, for we need the preservation of accounts of all the principal eclipses in order to complete the theory of the physical constitution of the sun. I request, therefore, that you will cause it to be published, if possible, in some of the works issued by the Smithsonian Institution.

Be pleased, sir, to accept the assurance of the respectful consideration of your humble servant,

EMM. LIAIS,
Astronomer of the Observatory of Paris,
Mission Scientifique, 56 *Rue de Belle-Chatte.*

Professor HENRY,
Secretary of the Smithsonian Institution.

On the eclipse of the sun, April 25, 1865.

[Extract of a letter from M. le Baron de Prados to M. Liais, dated April 26.]

In pursuance of your indications I repaired to Rio de Janeiro some days before the opening of the Chambers,* that I might be able to observe the eclipse of the 25th instant. Unfortunately, the sky remained overclouded up to the time of the first contact. When the sun could be observed, the shadow of the moon had already invaded its disk, so that the first contact was lost. The last exterior contact, the only one which I could observe with any exactness, took place, according to the observers who were present at the imperial observatory, myself being among them, at 11*h.* 54*m.* 5*s.* Being at the great meridian refractor, which had been removed in order to be directed upon the sun, I was enabled to follow those physical details which there was an opportunity of observing. The eclipse was not absolutely total at the observatory. A thread of light which, at the height of the phenomenon, took the form of a chaplet, perhaps prevented the observation of all the particulars of the corona. This last showed itself, however, for some moments in all its splendor. The following are the special circumstances which I was able to remark during the short duration of the phenomenon:

At the moment when the luminous thread assumed the chaplet form, the

* M. Baron de Prados is president of the *Corps Legislatif* of Brazil. He resides at Barbacena, where he has caused to be constructed at his own expense, and maintains, a large hospital for the poor. He studied medicine at Paris when young, and conducts the above establishment himself. The Chambers opened eight days after the eclipse, which explains the first phrases of the letter.

western border of the moon presented a magnificent ring of some seconds in breadth and of a violet-blue color. Its regularity was perfect. It was rather a luminous outburst of admirable effect. Nothing like it was manifested on the side of the eastern border. The ring of the corona was, nevertheless, well closed, and of a perfect pearl color, except on the eastern side, where the feeble line of solar light gave it the ordinary tint of the atmosphere near the edge of the sun. Five pencils of parallel rays of a perfect whiteness proceeded, almost perpendicularly, and without blending, from the edge of the ring of the corona. None of these pencils seemed to me contiguous to the lunar edge. If we except the violet-blue coruscation which showed itself on the western border of the moon at the height of the eclipse, nothing was observed which resembled those flames or protuberances which are almost constantly remarked in total eclipses, unless we suppose to be such the same magnificent luminous *trait* of violet blue of which I have spoken.

Perhaps the short duration of the eclipse, and the illumination, however feeble, of the eastern edge of the sun, prevented their being distinguished at our station. We shall learn what will be said on this subject by the expeditions of St. Catherina and Cabo-Frio.* Notwithstanding the instantaneousness of the phenomenon, I endeavored to verify the existence of the polarization of the light of the corona. For this purpose I availed myself of the polariscope with colored bands of Savart, and that of M. Babinet. It was with the former instrument that I best recognized the polarization. The bands were well colored on directing the instrument on the corona. The coloration was sufficiently sensible to forbid my admitting the intervention of the atmospheric polarization, for it was imperceptible when the instrument was directed on the lunar centre. It need not be said that the atmosphere was strongly polarized in all its regions, during the continuance of the phenomenon, in the manner in which it ordinarily is. One circumstance, manifested with much distinctness, was the visibility of the border of the moon beyond the solar disk during even the first phase of the eclipse. Arago, however, had remarked it in 1842, and you have also called attention to it in your observation of 1858 with regard to photographic tests by causing the solar image to fall upon unpolished glass. During the whole eclipse I carefully explored in the photosphere the solar surface which showed the greatest calm. By a singular defect the faculæ were scarcely perceptible in my instrument. Should the observations at St. Catherina and Cabo-Frio verify the absence of protuberances, the opinion will receive strong confirmation which supposes them to be formed by the ascending currents of solar vapors, which then involve by their impulsion the clouded extraphotospheric stratum, and whose violent elevation produces the protuberances. The photosphere was tranquil, and only a luminous line of a violet-blue color, a regular level stratum, presented itself to view. I sought with care for the existence of moving shadows. Nothing, however, was verified, although a large number of scholars of the Central school, who were then at the observatory, had their eyes fixed on the white walls of the cupola, favorably disposed for observation. The sky was so cloudy that we could perceive at our station only the planet Venus. The inhabitants, however, of places more to the south are said to have discerned several stars of the first magnitude.† The leaden color tending to violet predominated in the air and on the sea, which resembled molten lead. Domestic animals manifested the usual phenomena, the fowls seeking their roosts, while certain species of brutes seemed to manifest rather surprise than fear. Of the horses and mules in the streets of Rio de Janeiro, nothing re-

* Letters of a later date than that of Baron de Prados have informed us that these two expeditions encountered such bad weather as to preclude observations.

† To the south of Rio de Janeiro the eclipse, according to other information, was absolutely total.

markable was noticed. The meteorological observations offered the same anomalies which have been remarked in 1858; that is, the minimum of temperature did not correspond with the maximum of the eclipse. The temperature began to ascend immediately after the commencement of the phenomenon, and then sank until the latter was at its height, when it stopped at 24.3° centigrade. Before the eclipse the same thermometer marked 24.7°. The same thing occurred with the barometer, which commenced ascending at the beginning of the eclipse, and did not decline till 9*h*. 4*m*., reaching its minimum at the point of greatest obscuration. Having remarked nothing striking as to other meteorological phenomena, I limit myself to these simple indications.

REPORT OF THE TRANSACTIONS

OF THE

SOCIETY OF PHYSICS AND NATURAL HISTORY

OF GENEVA, 1861.

BY REV. M. DUBY, PRESIDENT.

TRANSLATED FOR THE SMITHSONIAN INSTITUTION BY C. A. ALEXANDER.

GENTLEMEN: It would afford me great satisfaction to be able to communicate, in the rapid sketch which I am about to present of the proceedings of our Society, some small portion of the pleasure which I have myself derived from a review of them. In the full and accurate reports of our secretary, the instructive lecture and animated discussion have seemed again to pass before me, and these I must now attempt to retrace, but, of course, without the hope of reproducing that which formed so large a part of the charm of our meetings—the uniform kindness which pervaded them, the unaffected urbanity with which each, whatever might be the line of his own studies, lent an attentive interest to the researches of his colleagues. The classification which I shall follow, in giving an account of your proceedings since M. Pictet read to you the last annual report, will be that adopted by him, as well as my other predecessors.

PHYSICAL SCIENCES.

You recall, doubtless, the interesting paper presented last year by M. Ritter, on the figure of the earth. He has lately resumed this subject. In his second memoir he has applied to the calculation of the dimensions and exact form of the globe the analysis which he had previously developed, while availing himself of all the observations which furnish the actual elements of the problem. His calculations extend over eleven arcs, divided into sixty sections, and comprising seventy-five stations, with a total amplitude of eighty-six degrees, which are not all contiguous. It results that the ideal metre, or the ten millionth part of the quarter of the meridian, exceeds, by two hundred and twenty-eight thousandths of a millimetre, or one hundred and one thousandths of a line, the legal metre, or metre of the archives. The flattening of the earth is $\frac{1}{292}$, with an uncertainty of 2.6 in the denominator. The equation of the meridian differs unquestionably from that of the ellipsis, the meridian being swelled out towards the forty-fifth degree by a stratum whose thickness is twenty-seven toises, with a probable error, more or less, of twenty-four toises. This uncertainty pertains chiefly to the latitude of three of the stations—Montjouy and Evaux, in the French arc, and Kamiez, in the arc of the cape.

To M. Ritter we also owe an account of the new experiments which the office of the ordonnance survey, charged with the geodesic operations of Great Britain, has caused to be made in Scotland, with a view to determining the density

of the earth. M. Ritter informs us that the manner in which these experiments have been conducted, and the possibility that unknown and unconsidered substances may exist in the mountain, on the two sides of which the experiments were made, do not authorize us to accord to the results obtained the same confidence which should be inspired by experiments of the nature of those of Cavendish. Again, the operations in England, conducted by the commission for restoring the standard of measures of length, (the *yard*,) have found in M. Ritter a reporter qualified to convey to his colleagues a clear idea of the difficulties encountered, and of the scrupulous precautions taken to obtain a solution of the problem.

The study of the periphery of our globe, and the phenomena it presents, have been the subject of several communications. M. Chaix, in giving a summary account of the voyage of McClintock to the polar regions, showed that the boreal lands have in general a higher relief than was heretofore supposed. The mean relief of the islands discovered since the voyages of Captain Ross reaches 2,000 feet. Different indications lead to the belief in recent upheavals. To the same colleague we are indebted for a sketch of a memoir by Colonel Graham, on the semi-diurnal tides of Lake Michigan, from which it results that the high spring tide at the syzygies rises to 3.48 inches, and M. Graham thinks would reach 4 inches, were all causes of disturbance removed.

Professor Wartmann and M. de Saussure on two occasions occupied the attention of the Society with a work by M. Thomassé, on the hydrology of the southern part of the United States. This latter savant, accepting the statement of American engineers that the quantity of water conveyed by the Mississippi equals but the tenth part of the whole quantity which falls in the basin of that river, contends that to explain this phenomenon it is necessary to suppose a drainage by subterranean passages, and attributes to that cause the fountains of fresh water observed in the sea at the mouth of the river. M. de Saussure cannot admit that these fountains proceed from cavities or clefts in the middle or superior portion of the river, which flows over the old sandstone, quite unconnected with the recent formations of New Orleans. M. Chaix disputes even the basis of M. Thomassé's hypothesis. Not only is it very difficult accurately to gauge the river at different seasons, but we are by no means in possession of the necessary elements for estimating, even approximately, the quantity of water which falls in the basin of the Mississippi. M. Chaix reminds us that M. Ellet gauged that river both below and above each of its great affluents, and that the result showed that the quantity of water conveyed, though augmenting considerably at each point of confluence, regularly presented a sensible diminution fifty leagues lower down. This diminution, according to the engineer just mentioned, is easily accounted for when we observe that below the Arkansas the right bank is low, swampy, and furrowed by bayous or arms of the river.

The natural glaciers of our mountains have been the object of very particular investigations by MM. Soret and Thury—by the former in reference to a glacier above Thun, and by the latter in the case of the Pré de St. Livrés, in the vaudese Jura, and in that of Vergy, in the Alps of Savoy. It was in winter that M. Thury made the visits of which he gave us an account, and he draws from his observations the conclusion that the time of the formation of the ice in these cavities must have been the season of the year when both water and frost prevail—that is to say, in autumn, and especially in spring.

Professor de la Rive presented to the Society copies of three Portuguese maps of Africa, of an earlier date than 1558, which were sent to him by M. Lavradio, for the purpose of showing that many geographical facts discovered within late years were not unknown at the above epoch. M. Chaix, in effect, called attention to the singular fact that these maps indicated a chain of lakes

and rivers in the interior of southern Africa. This circumstance, however, loses its importance when we observe that, although referred to the same latitudes with those discovered by Livingstone and Burton, the names borne by these collections of water betray the error by which, while really belonging to equinoctial Africa, they have been transported too far to the south.

Before quitting our own planet to recall the communications relative to astronomy, I should occupy a moment with an account of some researches respecting the atmosphere. To Dr. Lombard we are indebted for a memoir treating of the influence of altitude on rain. M. Gasparin, it will be remembered, claims to have established the law that the quantity of rain increases with the height. M. Lombard has collected, as bearing on this point, numerous observations published in the United States, and, having compiled and compared many tables, arrives at results which, whether as regards the valley of the Mississippi or the whole country, entirely contradict the supposed law. M. de la Rive, reminding us of a theory formerly advanced by himself regarding the formation of non-concentric hailstones, and ascribing it to the sudden congelation of collections of globules of water suspended in the atmosphere and cooled below zero, took occasion to announce to us that Professor Dufour, of Lausanne, has recently, by very ingenious experiments, furnished additional probability to the theory, and shown the effect which violent concussion would have in producing the phenomenon.

The principal discussions in regard to astronomy arose from the observations of the total eclipse of the sun, July 18, 1860, made by Professor Plantamour, at Castellon de la Plana, in Spain, and by Colonel Gautier, near Tarrazona. It was certainly a fortunate circumstance for the Society that two of its own members were among the accomplished observers of these striking phenomena, and the details furnished by our colleagues were received with marked attention. As their memoirs have been published, I shall not here attempt a detailed analysis; I shall only observe that the essential point on which the discussions turned was in relation to the red protuberances which, immediately after the disappearance of the sun, showed themselves on the edge of the obscure disk of the moon. Those observed by M. Gautier seem not to have been identical with those which M. Plantamour has so well described. The former particularly noticed one of these protuberances which, after having made its appearance at the commencement of the eclipse under the form of a small spot, continued to increase with a regular gradation and assumed the form of a large triangle, a little to the right of the zenith. But the chief subject of variance between the two observers regards the cause of these protuberances. If both agree in extolling the splendor of the spectacle, it is held, on the one hand, by M. Plantamour, to be a simple optical effect produced by the interposition of the screen which changes the direction of the sun's rays; while, on the other, M. Gautier thinks that the phenomenon is essentially solar. It would occupy us too long to state the arguments by which our learned observers sustain the conclusions at which they arrived. I shall merely add, on the authority of M. Gautier, that the author of the annual report of the Astronomical Society of London seems to have adopted the opinion that the protuberances pertain to the sun.

M. Gautier has from time to time supplied us with information respecting the researches of M. R. Wolff on the spots on the sun. These researches confirm the existence of a period of about eleven years in the return of the spots, but their size is modified in an interval of five or six of those periods. The elder M. Wartmann stated, with regard to these spots, that when observed directly through the telescope they seem black, but when the image is received on a screen their appearance is red. He thinks that the phenomenon in the former case is an effect of contrast with the light of the orb. M. de la Rive

called the attention of the society to some new experiments by M. Kirchoff relating to the influence exerted on the stripes in the spectrum of a flame by the presence in that flame of certain metallic substances. From these experiments highly interesting consequences regarding the nature of the solar atmosphere are deduced by M. Kirchoff.

From M. Gautier we also received an account, first, of a memoir of M. Otto Struve, jr., on the annual parallax of the stars, *alpha* of the lyræ and 81 of the swan. The result of the observations on this last star establish its comparative proximity to the earth, from which, nevertheless, it is separated by fourteen millions of millions of leagues; second, of a memoir of M. Powel, of Madras, on the double star *eta* of Cassiopea, the distance between the two stars being 7″, and their orbit indicating a revolution of 181 years; third, of the publication of tables of Venus, by M. Leverrier, the results of which indicate that the value of the mass of the earth is to be slightly augmented; fourth, of observations made in England and in Germany on a nebula which, during the month of May, 1860, assumed for some days the appearance of a brilliant star of the sixth or seventh magnitude.

M. Wartmann, sr., notified us of the discovery, between Mars and Jupiter, of six new asteroids. On this occasion he combated the idea of M. Leverrier, that these new planets might be recently formed from the cosmic matter diffused through space. He also communicated a note on an aurora borealis observed at Geneva March 9, 1861, in which it is shown that the theories of the aurora heretofore given leave unexplained the cause of the movement of oscillation which is executed by describing suddenly and completely an azimuthal arc of several degrees in extent to the right and left of the magnetic meridian. M. Wartmann invites the attention of theorists to this strange phenomenon, which equally concerns both physics and meteorology.

The communications relating to electricity have been, as usual, quite numerous. M. L. Soret presented on the 6th December an essay towards a mechanical theory of electricity. After having recalled the fact that electric phenomena, and especially the calorific and mechanical effects, seem to adapt themselves fully to an hypothesis like that on which rests the mechanical theory of heat, he infers that electric phenomena are to be regarded as molecular movements subject to the ordinary laws of mechanics; and he proceeds to investigate the nature of those movements which he considers to be rotary. The rotation may be executed in two directions, from left to right and from right to left. From thence would flow that duality which characterizes electric phenomena, and conducting bodies would be those which allow the transmission of the rotary movement of a molecule to neighboring molecules, while that property would be absent in isolating bodies. On these principles M. Soret explains the facts of both static and dynamic electricity; and he terminated this first communication by showing that the phenomena of the propagation of currents and extra-currents, of the closing and the rupture of a circuit, are easily explicable on the hypothesis thus presented. The chief objections to this theory arise from the impossibility of explaining thereby actions at a distance, and the consideration that the movement of rotation of a molecule cannot produce in the neighboring molecules a movement in the same direction, but necessarily movement in a contrary direction.

M. de la Rive reminded us that in 1849 he proposed to explain the variations of the magnetic needle by the existence at the surface of the earth of electric currents resulting from a rupture of equilibrium of the terrestrial and atmospheric electricity. This theory was rejected by astronomers, who maintained that the magnetic variations are too intimately connected with the position of the sun not to depend on the direct action of the mass of that body. Recently, however, a celebrated astronomer, Father Secchi, has anew had recourse to the

influence of atmospheric agents in order to explain to a great extent the variations of terrestrial magnetism. Every rupture of meteorological equilibrium producing a condensation of watery vapor would produce a rupture of electrical equilibrium. This equilibrium cannot be re-established except by currents of the surface, currents which must act on the magnetic needle. Doubtless the mass of the sun exerts a direct action on terrestrial magnetism, but M. de la Rive thinks that this action has been much exaggerated. It would not surprise him if in the magnetometer there were eventually found an instrument of meteorology at least as delicate as the barometer.

By the same colleague an account was given of the experiments relative to electrical cables, which he had witnessed in England. It appears that the ill success of the transatlantic cable is chiefly to be ascribed to the defective setting of the soldering, and also to the circumstance that the cable was laid in such a manner as was calculated to produce ruptures in the isolating envelope. Nor is the fact to be overlooked, which has been proved by direct experiments, that pressure increases the conductibility of the envelope of gutta-percha, which is not so isolating as has been generally supposed. During an excursion to the neighborhood of Dover, M. de la Rive took occasion to observe the application of electrical illumination to light-houses. The electricity is not generated by a battery, but by magnets of steel arranged on a circumference before which pass points of soft iron surrounded by a coil and placed on the periphery of a wheel. To this wheel motion is communicated by a small steam-engine. The cost of the apparatus once defrayed, and this cost is certainly considerable, the daily expenses are less than those of ordinary light-houses. We learn from a letter addressed to our colleague by M. Becquerel, who has since studied the subject, that the light obtained by the above means is very constant. A machine of one and a half horse power, consuming six kilogrammes of coke per hour, suffices to produce currents which, issuing between two retorts of charcoal, give a light equal to 300 wax candles or 70 carcel lamps.

Professor Wartmann reported to the society the researches of M. Magnus on the conductibility of gases. When a metallic wire is heated to redness by the current of a battery, it is found that the duration and intensity of the heat vary with the circumambient gases; hydrogen, for instance, conducting heat as a metal would do. M. Wartmann gave an account also of a memoir of M. Rike, of Leyden, designed to explain the non-instantaneousness of the propagation of the electric fluid in conductors. He compares the propagation of electricity to the efflux of water or of elastic fluids under certain determinate conditions. To conclude what relates to the principal reports concerning electricity, I may here mention that M. L. Soret presented the model of a new battery, constructed by M. Delenil, of zinc and protosulphate of mercury, which, being charged with pure water, exhibited for three months an action perfectly constant.

The properties of gas have formed the subject of several communications. M. Marcet called attention to some new experiments of M. Tyndall on their diathermal power. From these it results that simple gases absorb only 3 per cent. of the caloric emitted by the source of heat, while compound gases absorb much more considerable quantities of it; olefiant gas, for instance, 81 per cent.; oxygen and nitrogen combined in protoxide of nitrogen, 60 per cent. Vapors absorb more than gases. M. Marcet likewise cited a memoir of M. Franckland on the influence of the rarefaction of air upon combustion, and showed that it may be considered as nothing. On the other hand, the intensity of light diminishes rapidly with the density; this loss of brightness being, in England, 0.05 to the inch of the pressure of mercury. M. L. Soret repeated before the society an experiment of M. Deville, intended to show a property of endos-

mose of gases in traversing porous earths. This experiment evinces that if when a current of hydrogen is traversing a tube of baked earth, we stop the current, there is a vacuum produced in the apparatus, which can be only attributed to the circumstance that a portion of the hydrogen passes through the sides of the porous tube. At the same time a certain quantity of atmospheric air is mixed with the gas remaining in the apparatus.

M. Antoine communicated new researches on the combustible part of the gas of the fumarolles of Tuscany. It is composed in variable proportions of the marsh gas and protocarbonated hydrogen of double the volume. He concluded with some general considerations respecting the absorption of gases. From his experiments it results that the complete solution of the gases, on which liquid reactives exert a special action, depends on the mass of the absorbent body and on the extent and duration of the contact. M. de la Rive announced that M. Schönbein, who has been long occupied with the isolation of antozone or positive oxygen, has arrived at the desired result by the trituration of fluor-spar in water.

M. Marcet gave an account of a memoir, published in America by MM. Elliot and Scherer, on the purity of zinc. The purest of all is that of Old Mountain, and next the zinc of Pennsylvania. On this subject, M. de la Rive announced that M. Deville has succeeded in obtaining very pure zinc by means of distillation, and that he has, moreover, discovered a process for procuring it in a very pure state from the sulphate of that metal. M. Favre referred to some new experiments of M. H. Deville for the production of artificial minerals. He has succeeded in producing fluoride of aluminum and staurotide, and has ascertained that a very small quantity of the fluoride of silicon will mineralize a very considerable mass of base. M. P. Morin communicated an abstract of an analysis which he has made of water from the fountain of Guillot at Evian. The results are much the same with those obtained by MM. Tingey and Peschier from the water of Cachat. In the present case special attention was paid to the glairine and the bituminous substance contained in the water of Guillot.

But one communication has this year been presented on the subject of light, and for that the Society is indebted to a young physicist who is not one of its members. M. Lucien de la Rive favored us, on the occasion spoken of, with an account of a new experiment on parallel diffraction, in which he had studied the image of the sun. As he has announced his intention of presenting a second memoir on this subject, I shall attempt no analysis of the former, more especially as it would involve details which the limits of this report will scarcely permit. For a like reason, and because it has been already printed in the *Bibliotheque Universelle,* I restrict myself to a single notice of the paper read by M. Thury to the Society, entitled, "Remarks on an article of Silliman's Journal, relative to Spencer's microscopes and the structure of the wood of the coniferæ; and considerations on microscopes in general." We all remember how many important remarks on microscopes, and how many practical instructions, as precise as they are useful, with reference to the present state of those instruments, are contained in the paper in question. As to the pores of the coniferæ, contrary to the figures given by the American author, M. Thury has satisfied himself, by direct observation, that the thin membrane which, according to some naturalists, forms the base of these pores, really exists, and the organization remarked by M. Clarke in the old wood can be only the result of an alteration of the organs.

NATURAL SCIENCES.

In commencing a review of this branch of the occupations of the Society, geological communications are those which first present themselves. Professor Favre, having lately visited Amiens in Picardy, gave the results of his explo-

rations, and submitted some of the instruments cut from silex which are found in the quaternary formations of that precinct. These axes, as they are called, occur in the bed of gravel which also contains the bones of animal species now extinct. In the bed of white sand which overlies the stratum containing them he met with a geode composed of well-defined crystals of hyaline quartz. The position of these crystals, as M. Favre maintains, shows that they were formed since man has inhabited the earth. This recent origin of the quartz would explain how crystals of that substance come to be found on a projecting point of one of the axes exhibited to the society, since they could only have been formed after the translation of the gravel and the axes.

From the same colleague we received an account of some geological observations which he has recently made in Maurienne. On the right bank of the Arc, between St. Jean and the pass of Encombres, he found that the formations were folded or bent like the bottom of a boat, while the rock of the coal formation comprised between the former locality and the tunnel of the Alps presents, on the contrary, a sort of fan-like structure, and a vault in the eastern part of this vast group. From these observations he concludes that the formation which contains the anthracites between St. Michel and Modane pertains to the true coal formation, that it is covered by the triassic rock, and that the liassic and nummulitic strata occupy, in relation to the other formations, the same position as in other countries.

A statement was also given by M. Favre of the observations and experiments of M. Daubrée on metamorphism, whereby the latter has shown that, in explaining this class of phenomena, the action of water is to be taken largely into account. With these results of M. Daubrée our colleague collated the discovery of M. Sozby of the existence, in all granitic quartz, of myriads of small cavities, filled, some with gas, others with liquid. He further called to our notice a discussion which had arisen in the Geological Society of London. M. Murchison has observed, over a great extent of Scotland, gneiss resting upon quartz, even argillaceous schists and limestone resting upon granite. This immense group could not owe its origin to a local inversion of strata, like those observed here and there in the Alps. M. Nicol earnestly contested the existence of this overlying gneiss. This report gave occasion to M. de Saussure to remark that M. Logan has described this superposed gneiss as existing in Canada to such an extent as to exclude all idea of an inversion.

At our last meeting, M. Favre recounted an excursion which he had made with M. de Morlot to the cone of erosion of the Tiniere, near Montreux, and explained the theories of that savant on what he calls the Roman deposit, four feet below the present surface; the deposit of the age of bronze, six feet below the former; and, ten feet lower still, the deposit of the age of stone; together with the reasons on which he founds those distinctions and names.

The Society has been favored by Professor Pictet with numerous communications relative to paleontology, of which the following are the most important. First, a notice on the succession of cephalopod molluscs, during the chalk period, in the region of the Swiss Alps and the Jura. He derives from a detailed study of the fossils contained in the cretaceous strata of Ste. Croix, and their comparison with cotemporaneous repositories, an argument in favor of the idea propounded by M. Barrande, that two successive faunas must necessarily have existed together for some time, and he concludes by showing that paleontological faunas distinguished throughout by marked characters are not ordinarily susceptible of any rigorous limitation. MM. Claparede and Favre took occasion to remark how much the conclusions of M. Pictet must in future complicate the task of the geologist who undertakes to determine the age of a formation.

Led by his study of the neocomian fossils to determine a great number of fragments of unrolled cephalopods, M. Pictet has attentively considered their

septa, and has found that there is no connexion between the form of these septa and the generic characters. On the contrary, he has arrived at this unexpected fact that the septa have undergone what might be called a sort of geological evolution. Their form is a character not of the species, but of the epoch in which the cephalopod was alive. M. Pictet recognizes the two following laws: 1. All the neocomian species have the superior lateral lobe divided into unequal parts; 2. The proportions of the inferior lateral lobe vary with the geologic age. M. Pictet further informed us that near Montiers, in Switzerland, occurs a site analogous to that of Mauremont, formed, that is to say, by the fauna of the basin of Paris, in superficial rents or fissures. Below, in the jurassic formation, bones of the Megalosaurus have been discovered; a fact of interest, because that reptile constituted, with the Iguanodon and the Ileosaurus, the only great terrestrial reptiles of the jurassic period. At Mauremont have been found a jaw-bone of the Rhagatherium and a tooth of the horse and ox, respectively—teeth, which have evidently proceeded from an intermixture later than the eocene fauna, which, till now, has been found in Switzerland free from all accessions.

We owe to the same colleague an analysis of a memoir by M. Desor, relative to the question of the fossil man. The author combats the opinion pronounced by M. Pictet, that there has been no appearance of new species since the commencement of the quaternary epoch, but only extinction of species. To prove that during the quaternary period new species have appeared, M. Desor remarks that certain fishes, particularly the *Cyprinus idus*, are found only in the lakes of the north of Italy, and these lakes having been filled with ice during the glacier period, the creation of the fish must have been subsequent. M. Pictet refutes this remark by observing that when two identical fishes are found in two basins without communication, we have recourse, in order to explain this fact, to any other hypothesis rather than that of a special creation for each basin. On this occasion M. de Candolle called attention to the great difficulty which exists, in some cases, of comprehending how certain aquatic plants could have reappeared on our lakes after the glacier period; though M. Wartmann was of opinion that their seeds might during the interval preserve their germinative faculty.

M. de Saussure exhibited the skull of a stag, found in a canal at Longmalle (Geneva,) and presenting a striking peculiarity; it bears the mark of blows given with cutting implements, and the antlers have been separated by a stroke of a hatchet. It is probable that this skull was buried in the sands on the border of the lake in the same manner with the bones which are taken from the lacustrian sites. Our colleague also submitted to the Society some observations made in the pass of Bernardino in Oregon. Here are to be seen vast extents of rocks smoothed as if by glaciers, though without striæ; the smoothness being attributable to the action of the sand which, in those regions, the wind transports in great quantities, while all the edges of the rocks exhibit channellings in the direction of the prevailing wind.

M. de Loriol, then a free associate, presented a memoir on the fossils of the middle neocomian of Salève. This neoçomian has a peculiar *facies*, characterized by the *Ammonites radiatus*, which might be named the jurassic *facies*, because all the deposits of the Jura pertain to it. The neocomian of the Voirons and of Mole pertains to the alpine *facies*, with the deposits of the Alps, which possess characteristic fossils. These two *facies* often meet side by side in the south of France. For four years MM. Pictet and Loriol have been collecting numerous specimens of the fossils of Salève. The latter has distinguished 138 species of invertebrata pertaining to Molluscs, Annelida, Echinidæ and Spongiaria. Thus far he has met with no Polyps. In general the preservation of these fossils is imperfect, specimens invested with their shells being

seldom found. A large number of inside moulds, especially of Mollusks, offer a singular peculiarity. Certain portions of their surface are covered with serpulæ and encrusting bryozoa, which have evidently lived on these moulds. The middle neocomian of Salève may be divided into six strata, which present, paleontologically, certain differences, but which contain an assemblage of fossils pertaining to two distinct faunas, both carefully described and characterized by M. Loriol.

In answer to some objections of M. Favre to the epithet *alpine* as applied to the neocomian *facies* of the Voirons, M. Pictet pointed out that there are in the neocomian two very distinct faunas; one of which occurs in the Jura and throughout France; the other, whose fossils are wholly different, commences at Sentis, traverses the small cantons, the Bernese Oberland, the canton of Fribourg, and extends to Châtel St. Denis and Bex. It is again found at the Voirons, at Mole, whence it stretches along the Isere, traverses the higher and lower Alps, and following the prolongation of these mountains reaches Padua and Venice. This fauna is characterized by great numbers of unrolled cephalopods. There are points of contact where these two faunas meet, as if by digitations. Salève forms a jurassic digitation, the south an alpine digitation.

Botany has not, this year, played any considerable part at our meetings. M. Claparede recounted the new experiments of M. Pasteur on fermentation. This savant has observed that the infusory animalcules which are developed in fermenting liquids continue to live when deprived of oxygen. But in the opinion of all microscopists, these pretended animalcules are in reality vegetables, which should be classed with the semi-cellular algæ.

Professor Wartmann communicated the result of experiments which, at the request of M. Thury, he had made on the influence which excessive cold exercises upon seeds. Seeds, some of which had been exposed for a half hour to a temperature of 57° centigrade, and others for twenty minutes to one of 110°, vegetated, when sown in spring, as well as the seeds of the same species which had been protected from cold. It results that the greatest cold we can produce does not destroy or even enfeeble the vitality of seeds.

M. Casimir de Candolle, who was not then a colleague, read a memoir on the artificial production of cork, which he has had an opportunity of observing during a sojourn in Africa. This paper having been printed in the sixteenth volume of the Memoirs of the Society, which will contain the report I have now the honor to present to you, no analysis of it is necessary. Your president has also had the privilege of making some communications to you. I submitted to you my researches respecting the family of the Hypoxyleæ, (*Pyrenomycetes, Fr.,*) and endeavored to show that to this entire group of fungi should be applied the same principles of classification which I have adopted for the tribe of the Hysterineæ. This memoir being but the development of § 4 of the paper which you have caused to be printed in the sixteenth volume of our collection, I refer to that paragraph all who may be interested in the subject. I took occasion to bring to your notice the observations of M. de Bary on the *Cystopus candidus*, a minute Uredinea, which forms white spots on the leaves of the Scorzoneras, in the spores of which the Professor of Fribourg tells us he has seen, when they are sown in water, the formation of zoospores furnished with two flagellary cilia. M. de Bary also tells us that, in certain conditions, he has seen zoospores formed in the tubes issuing from the spores of the champignon of the potato, (*Peronospora devastatrix.*) I communicated to you, in the last place, the researches of M. Hicks on the gonidia of Lichens, from which it would seem to result that a multitude of pretended aerial Algæ, described under the names of *Protococcus, Palmoglœa, &c.*, are but stages of development of these gonidia.

Some very interesting communications on zoology have been received by the Society in the course of the year. By M. Claparede our attention was called

to experiments conducted in London, by M. Marcet, with a view of determining whether the toxic action of alcohol makes its impression on the brain through the medium of the circulation or of the nerves. The experiments in question show that the circulation is the essential intermediary. Our colleague further laid before us the result of researches, in which he has been engaged for three years, on the evolution of arachnida in the egg, and accompanied his developments with several plates. A detailed analysis of this remarkable memoir would involve so many particulars, and necessitate the use of so many technical terms, that I am with reluctance compelled to forego it. The result of the phenomena imports that the embryo originally rolled up on the back is in the end rolled up on the belly. To this memoir M. Claparede added some expressions on the utility of these embryological investigations in the comparison of the appendages of the spider with those of other arthropods. He shows that the protognaths or forciples of the spider are homologues of the antennæ of the larvæ of insects and of the antennæ of the second pair of the crustacea, while the deutognaths are homologues of the mandibles of the crustacea and of insects. M. Claparede also presented us with some drawings of animals but little known, which he had observed in the Hebrides. The first of these are representations of two species of worms of the group of the Sipunculoidæ. Another represents an *Actinotrocha branchiata,* a marine animal for which hitherto no place has been found in any division of the animal kingdom. M. Claparede suspects this strange looking creature to be the larva of a worm. Lastly, our colleague gave us an account of the physiological researches of M. Voit in regard to the pearl Unio, and explained the reasons which render doubtful the respiratory functions of the pretended branchia of the Lamellibranch.

M. de Candolle presented, on the part of M. Guerin-Menneville, a report on the attempts made for the acclimatation, in France and Algiers, of the silk-worm of the Ailanthus, *(Bombyx cynthia.)*

M. de Saussure continued his communications on the natural history of Mexico. He spoke first of the Vampiridæ, cheiropterous animals with a peculiar membranaceous foliation seated on the nose, and showed, as specimens, those particularly of the *Mormopsis Blainvillei,* which, till now, were found in no museums but those of London and Berlin. He afterwards described a *Centurio,* with a face singularly distorted, and withal more obtuse than that of any of the Mammals, man included, but with a skull so greatly flattened as to produce a resemblance to the frog. M. de Saussure further presented a Coati, of which a new species of late has been erroneously formed.

From M. Humbert the Society received some very interesting statements with respect to his explorations in Ceylon; and first, in regard to many species of terrestrial Planariæ, of which he exhibited drawings, and several of which, discovered by M. Humbert, are distinguished by the malleiform or crescent-shaped enlargement of their anterior portion. M. Claparede followed this communication with some remarks on the anatomy of one of these species. M. Humbert proceeded to give some particulars respecting the land leeches, which are very common, and not a little inconvenient in certain parts of the island—so common, indeed, that it is impossible to keep goats in such places. At another meeting he gave an account of the general aspect of the ornithological fauna of Ceylon, and exhibiting numerous specimens, described the habits of the curious birds which he placed before us.

I shall have concluded the portion of my report which relates to zoology when I mention the memoir read at our last session, by M. Victor Fatio, on the different varieties of frogs which frequent the environs of Geneva. In this memoir, which is accompanied with colored plates, the author describes, under the name of *Rana gracilis,* a new species found in the marshes of Puplinge, and produces some fine specimens of it.

The foregoing is but a colorless sketch of the transactions of the Society during the year which terminates to-day.* Yet, however incomplete, it may suffice to show that among the members of this association the love of science and of labor has not diminished, and that it continues to procure, for minds capable of appreciating them, those pure and elevated pleasures of which the study of the works of the Creator is an inexhaustible source. May He who has placed us in a country so rich in subjects of research and meditation continue to preserve you, that by your example and instruction you may encourage those who follow you in life to strive likewise to lift some of those veils which still hide from us so many mysteries, and worthily to fill the vacancies which the will of God accomplishes in our midst.

We have this year had but a single loss to lament—that, namely, of our senior member, Prof. Maunoir. We have not of late seen him at our sessions, but the former records of the Society attest the interest with which he regarded its proceedings. In the brief sketch which I can here give, and while regretting that the office of commemorating his eminent merits has not fallen to some one better qualified, I shall be held excused if I touch rather on those works of our deceased colleague which bear a direct relation to the physical and natural sciences than those pertaining to the art which he practiced with so much distinction.

Jean Pierre Maunoir was born October 10, 1768, at Geneva, and was led at an early age, in the pursuit of his surgical studies, to visit first Paris and afterwards England. The first publication by which he became known was published in 1812, and was entitled *Physiological and practical memoirs on aneurism and the ligature of the arteries.* His reputation was still further extended by his writings on the organization of the iris and the operation for artificial pupil, in which he demonstrated that the iris is composed of a double muscular system, of fibres disposed in radii, proceeding from the larger border of the iris towards the centre of the pupil and of circular fibres surrounding the pupil like a ring. He applied his views on the function of these different fibres to the operation for artificial pupil, and succeeded in a great number of cases in obtaining pupils whose form was exactly that which was indicated in advance by the arrangement of the muscular fibres divided. The reputation which these publications procured him was advanced to a still higher point by the address, the skill, and the presence of mind which he brought to such delicate operations as those which regard the sight. I pass over various dissertations by our colleague on subjects of surgery, on the medullary fungus and hematodes, (1820,) on hydrocele of the neck, on amputations and immediate reunion, on cataract and the means of remedying it, in order to notice an essay published in 1842 on the adjustment of the eye to different distances. He attributed this faculty to a change of form in the ocular globe produced by the contraction of the external muscles. The recent progress effected in the anatomy and physiology of the eye no longer authorizes us to admit this conclusion, but teaches us to seek the explanation of the phenomenon in the interior of that organ and in the movements of the crystalline. The diversified labors of Maunoir procured him many honorary distinctions. Without speaking of the place he occupied in the scientific societies of Geneva, I shall merely recall that he was a correspondent of the French Institute and an associate of a great number of learned societies of the highest distinction in Europe and even in America. He loved science in all its branches, and those of us who remember his attendance at our meetings will not have forgotten the intelligent

*(June 13, 1861.) It should be added, to justify more fully the expressions above employed, that many publications by members of the Society, and some of them among the most important, are not mentioned at the meetings.

interest with which he addressed his inquiries to the authors of the memoirs there read, in order to develop a clearer idea of the theories advanced or the facts recounted. Nor will they fail to recall the kindliness of his manners, the promptness to oblige, and the perfect good-will which marked all his conduct toward his colleagues. It was on the 17th of January last that he terminated, at the age of 92, his long and admirable career.

The loss thus sustained by the Society has found a compensation in the accession of two new members, MM. Perceval de Loroil and Casimir de Candolle. In connexion with the latter nomination, it may not be amiss to say that no more agreeable duty could have attended the discharge of my functions as president than that of signing the diploma of membership of the son of the excellent colleague who is to succeed me in this chair, and grandson of the distinguished master who once honored me with his counsels, his instructions, and his friendship.

After what was said by my predecessor, M. Pictet, in the annual report of 1860, respecting the incorporation into our body of the *Free Associates*, I need only add that we have had no cause to regret this change in our organization. The regular attendance of several of them has contributed much to the interest of our sessions, and we may hope that they will in future take even a still more active part in our deliberations and enhance their favors by occasional communications. Alas, while writing these lines I cannot forget that two of the names which we had gladly inscribed on our lists will next year be no longer found there, and that the void which they leave is but an imperfect symbol of the greatness of the loss, not only to their families and friends, but to the community and the country. The two free associates whom we no longer possess are the former syndic, M. Barde, and Dr. Rilliet.

REPORT ON THE TRANSACTIONS

OF THE

SOCIETY OF PHYSICS AND NATURAL HISTORY

OF GENEVA, 1862.

BY PROF. DE CANDOLLE, PRESIDENT.

TRANSLATED FOR THE SMITHSONIAN INSTITUTION BY C. A. ALEXANDER.

GENTLEMEN: In presenting, as a rule of the Society requires, a review of its transactions during the past year, I find my task singularly facilitated by the exactness with which your proceedings have been recorded by our learned secretary, M. Claparede. Yet I am sensible that, as regards the relative length of the articles, the same rule will not apply to his labors and to mine. An analysis, however abridged, of memoirs already published or about to appear in the forthcoming volume of our series, would be little less than absurd. Our reports are addressed to men of special culture, who prefer to consult the original work, and are by no means embarrassed by the necessity of recurring, for instance, to the *Bibliotheque Universelle*, or the *Memoires* of our Society. I shall, therefore, simply indicate in many cases the place where dissertations are to be found which occupy a large space in our records, while other motives will induce me to pass rapidly over certain communications which possessed, at the moment, a lively interest. I speak now of the information given respecting discoveries made in various countries respecting new works and the opinions delivered by learned men on controverted subjects. Communications of this nature, always numerous and varied, while they keep us apprised of the progress of science, and assist us in forming an opinion of men and things, have in them either nothing original, or if anything original and of intrinsic interest, it consists in the opinions pronounced regarding some work or method or theory; and it is obvious that, in order to remain habitually frank and unreserved, appreciations thus verbally given should not be made public. One of the charms of our association consists in conversation upon our own labors or those of learned foreigners. Let us preserve for this part of our exercises the advantage of being undivulged; it is one which, to a society like ours, limited, confidential, and unofficial, it would be annoying to lose. I shall proceed now to enumerate the series of our labors, classing and abridging them, and also suppressing much for the reasons just assigned.

ASTRONOMY.

To Professor Gautier we are indebted for an account of the observatories of Zurich and of several cities of Germany which he has had an opportunity of examining in detail. He has also, in numerous and varied communications, kept us informed of the advances made in astronomy. His views respecting comets* have been developed and completed in reference to an article in the

* *Resumé de divers travaux récents relatifs aux comètes.* (Biblioth Univ., Archives des Sc. Phys. et Nat., Fevrier, 1862.)

Bibliotheque Universelle, published during the present year. Colonel Gautier also favored us with observations made by himself, particularly those relative to the solar eclipse of 1860; while Professor Plantamour has continued to insert in our *Memoires* the series of astronomical observations made at the observatory of Geneva.

PHYSICS.

Is it among cosmic phenomena, or those which fall within the province of terrestrial physics, that we should class the *aurora borealis?* This question is no longer an open one since our learned colleague, M. de la Rive, has made it the subject of his profound research. On different occasions, and especially in an extensive memoir now in course of publication in our collection, he has furnished the proof that the boreal and austral auroras are a phenomenon produced in the highest region of the atmosphere by the encounter of opposite electricities. The higher region is habitually charged with positive electricity, while the earth is ordinarily negative, and the lower stratum of the air acts as an isolating medium. The winds drive the electric vapor towards the two poles where the discharges take place. M. de la Rive holds that, according to the laws of terrestrial magnetism, an equal conductibility of the maritime or wet surfaces being assumed, the manifestations ought to be simultaneous at both poles, and he insists that observation has in fact confirmed this simultaneousness. In order still better to demonstrate his theory, our ingenious colleague has had an apparatus constructed representing the terrestrial globe, and so arranged as to be capable of realizing all the conditions of magnetism; and with this, by the application of electricity, he has produced the different phenomena of the aurora, its glimmering light, its luminous jets surrounding the poles, &c., exciting the admiration even of those who take little interest in the theoretical question or the difficulties necessary to be surmounted in procuring so decisive an imitation. In this apparatus, constructed at Geneva, in the *atelier* of Professor Thury, and under the direction of M. Eugene Schwerd, a sphere of wood represents the terrestrial globe. It is so contrived as to present at the extremities of its horizontal axis two magnetic poles, around which the discharges of a Ruhmkorff apparatus produce luminous effects. The surface of the globe having been moistened, is covered here and there with small metallic plates, from which proceed wires terminating in a galvanometer at some distance. The deviations of the needle, when the polar discharges take place, are analogous, in their minutest phases, to those manifested in the common telegraphic apparatus during an aurora borealis. A peculiar arrangement allows of the artificial reproduction of the perturbations of the magnetic needle which accompany the auroral phenomenon. The memoir of M. de la Rive comprises a discussion on the nature of these perturbations and on the direction of the electric currents to which terrestrial magnetism is attributable. It is enough to announce these investigations to attract to them the attention of physicists.

M. Wartmann, senior, being, in September last, at Cologny, and therefore at a certain elevation above the left bank of the lake, several times observed, half an hour after the setting of the sun, singular effects produced by mirage. On the other side of the lake, or rather towards the middle of it, an island would make its appearance, presenting ranges of trees in a reversed position, while beyond this isle the lake retained its usual liquid appearance.

Professor Wartmann, junior, repeated before the Society the recent experiments of M. Plateau on bubbles of soap, of varied forms as well as much persistency, obtained by mixing with soapsuds a small quantity of glycerine, and causing the bubbles to attach themselves to iron wires arranged in different manners. At a subsequent session M. Wartmann exhibited an apparatus of the same kind, still more varied, so as to produce more perfectly than by former

processes the phenomena of coloration in extremely thin surfaces of the liquid. The dark part presents not more than $\frac{1}{104920}$ of a millimetre, whence we may conclude, says M. Wartmann, that the radius of the sensible activity of molecular attraction is below $\frac{1}{200000}$ of a millimetre.

M. de la Rive submitted to inspection a minimum thermometer of Casella, an ingenious instrument, in which, instead of a movable index, there is a lateral reservoir adroitly constructed, into which the mercury flows when it rises. M. P. Plantamour described to us the injector of Giffart, designed as a substitute for the supply pump in steam-engines. M. Eugene de Morsier exhibited a crust taken from boilers of those engines, which is impenetrable by water, and thus gives rise to accidents.

Our regretted colleague, M. Ritter, of whom I propose presently to speak, had presented to the Society a curious memoir* on the gamut of the mathematicians compared with that of the musicians. M. Alexander Prevost† subsequently analyzed this memoir, and, after deducing from it certain consequences, proceeded to compare them with the opinions and practice of musicians. Both memoirs having been published, we merely indicate them to the savants who interest themselves in questions of this nature.

Professor Plantamour and M. Hirsch, director of the observatory of Neuchâtel, have commenced a series of observations to determine the relative position of Geneva and Neuchâtel, by availing themselves of the electric telegraph. The highly improved instruments would have yielded satisfactory results had not causes arising out of the state of the sky and of the telegraphic lines interrupted the proceedings. The observation from Neuchâtel to Geneva gave a difference of longitude amounting to 3′ 12″.22; but when the observers would have changed their stations, in order to eliminate the personal equation, the sky had become overcast, and a derangement of the line prevented a prosecution of the work. The meteorological observations of the Great St. Bernard, compared with those of Geneva, have long been a subject of investigation, but successive improvements in the systems and processes of observation had rendered desirable a comparison after the lapse of the last twenty years. This has been undertaken by M. Plantamour in a first memoir relative to St. Bernard, published in the *Bibliotheque Universelle*, January, 1862, under the title of *Notes on the periodical variations of temperature and atmospheric pressure at the Great St. Bernard.*

A memoir by M. Ch. Martins, on the increase of temperature during the night, above the surface of the earth to a certain height, has given occasion to M. Marcet, who had been previously occupied with this subject, to make new experiments. They have confirmed certain differences which he had remarked between his own facts and those observed by M. Martins at Montpellier. A new series of observations was undertaken by M. Marcet with the view of determining whether the decrease of temperature exists above an aqueous surface of great extent. He ascertained that the phenomenon does not occur above water, and that it is even scarcely sensible in the immediate neighborhood of a large liquid surface, so that in effect there is, at the moment of the setting of the sun, a difference of from 2° to 3° between the temperature at a certain height above the land and above the water. The memoir of M. Marcet was published in the *Bibliotheque Universelle*, November, 1861.

M. Soret has visited the glacier of Shafloch, which M. Thury had examined the year before, and remarked the aureola structure previously noticed. He observed, moreover, that the entire surface of the ice was covered with small striæ nearly parallel in each aureola, but not having the same direction in the different fragments. These small striæ, which were also observed by M. Soret

* *Memoires de l'Institut Genevois*, in quarto, vol. viii.

† *Biblioth. Univ. Archives des Sc. Phys. et Nat.*, April, 1862.

in some parts of the ice of the glaciers, might be compared, as regards appearance and size, to those presented by the skin at the extremity of the fingers.

The river Orbe, from observations of M. Chaix, has not the same temperature with the lake of Brenets; the lake showing, July 4, 1861, 18° at a rather shallow point, and the Orbe 11° at the point of its issue. This is attributed by M. Chaix to four affluents, with a temperature of about 6½ degrees.

General Dufour brought to the notice of the Society operations in progress on the territory of Switzerland for the measurement of an arc of the meridian directed from northern Germany towards Italy.

M. Henri De Saussure exhibited a chart of the environs of Mexico, drawn from observations of his own combined with former maps.

GEOLOGY AND PALEONTOLOGY.

Several geological charts in course of publication have been communicated to us. One of them is the topographical chart of the canton of Vaud, on a scale of $\frac{1}{50000}$, which the government of that canton has ordered, and the coloring of which on geological principles has been committed to M. Renevier. Another relating to the geology of Savoy has long occupied the attention of M. A. Favre, who favored us with a view of the topographical chart which is to serve him as a base, as well as a chart of the environs of Mont Blanc, geologically colored by hand. An account was given us by the same member of the meeting, last year, of the Geological Society of France, in Savoy, and of the researches of M. Heer on the climate and vegetation of the tertiary epoch, a subject which has also been discussed by one of our own members in the *Bibliotheque Universelle.** We are further indebted to M. Favre for a view of certain plates designed to popularize the ideas and facts of geology and physical geography among the English people. Among communications relating to the pursuits of different savants, I must not forget an interesting account by Dr. Claparade,† of observations made in Sweden and Norway, on the succession of levels in the Scandinavian peninsula, and on the crustacea discovered at the bottom of lakes, after existence had been predicted; the lakes having been heretofore in communication with the sea, and being even below it. These crustacea have been found to be analogues of, or similar to, species existing further north in the polar seas.

The mountain of Salève, explored by Deluc, De Saussure, Necker, Alph. Favre, and so many other geologists, and which is known among the Genevese as "*the* mountain," so great is the partiality with which they regard it—this mountain has in the course of the year been the scene of unexpected discoveries and observations.

In their casual walks, MM. Grasset, Chomel, and Revon had observed four caverns situated above the village of Coin, and had brought away some bones and fragments of pottery, which seem to belong to the epoch called the age of bronze. Professor Thury, on this discovery, caused excavations to be made in one of these grottoes, and found, at the depth of half a metre, the remains of a fireplace or hearth, with tracks leading to it. He supposes that some of the ancient inhabitants sought refuge in these caverns, to escape the consequences of an invasion; and he proposes to prosecute his researches to more decisive results. It is the position of Salève, however, which is perhaps the most extraordinary feature of the mountain. It shuts in our valley with a lofty calcareous rampart, forming a natural limit, which the policy of states has alone refused to recognize. M. Favre has determined the cause of this abnormal position. Salève stands in the continuation of a great anticlinal line—that is, of a line of

* M. Alph. de Candolle, May, 1862.
† *Biblioth. Univ. Archives*, April, 1862.

dislocation and *plissement*, distinguishable from the banks of the Isar, in Bavaria, as far as Lausanne, and continued, according to M. Favre, by the uplands of Boisy, in Chablais, and by Mount Salève. The direction of these points is in a right line, as M. Favre has shown on a geological chart of Switzerland. Thus the molasse has been cleft throughout this whole line, and the subjacent jurassic limestone been lifted up—at no point, however, so high as on the site of Salève.

One of the last excursions of M. Favre was around Mont Blanc, in which he especially examined the Bas-Valais, and found at Mont Chemin, near Martigny, belemnites pertaining to the lias, which are found above the *cargneule* representing the lias. He found on the mountain of Maya, near the col Ferret, echini and fragments of the branches of encrinites which characterize the upper jurassic formation. The deposits in question are at about fifty feet from granitic rocks of Mont Blanc, and are covered by considerable masses of crystalline limestone.

Professor F. J. Pictet read a memoir on the unrolled ammonites of cretaceous formations. Till now these fossils have been found only in fragments, and it was easy to attribute to two distinct kinds, portions of the same animal. The author has pointed out many similar errors, which he has been enabled to correct by means of more complete specimens. This memoir will form part of M. Pictet's work on Swiss paleontology. In a note on the *parallelism of the middle and upper cretaceous faunas*, M. Pictet has aimed to show that the distinction made by Orbigny, of eight cretaceous stages, although holding good as to large tracts, is insufficient for the study of details and of the succession of faunas. He compares the middle and lower stages of the Swiss Jura, the northwest of Germany and the south of France, and confirms the observation of M. Lory that the neocomian faunas differed according to their geographical position. Not that they have varied uniformly over their whole extent. By the side of numerous analogies we see local differences which evince either migrations or different, although simultaneous, physical influences. In the discussion which ensued upon the reading of M. Pictet's memoir, some difficult questions of science were broached. Among other things, the proximity, sometimes considerable, of analogous formations furnished with different fossils, was spoken of, and facts of the same nature were pointed out by M. Claparede in the distribution of existing faunas, according to the depth of closely neighboring arms of the sea.

The curious discoveries of M. Lartet, tending to prove the antiquity of the presence of man in western Europe, were the subject of communications from M. Pictet, which he has published in the *Bibliotheque Universelle* for 1861.

Professor Thury stated his objections to the opinions of M. Marlot on the time occupied in the formation of the cone of Tinere, at the east extremity of Lake Leman. He has attentively explored the valley whence proceeds the detritus borne towards the lake, and he does not think that it can have furnished materials with the regularity supposed. On the contrary, it appeared to him that the quantity of material removed from the surface must have frequently varied.

ZOOLOGY.

M. Pictet exhibited moulds from two skulls of gorillas, whose diversity is sufficiently great to induce a suspicion of the existence of two distinct species. Dr. Dor presented important memoirs on vision, as well in man as in certain classes of animals. The author attributes myopy (short-sightedness) to a too great length of the axis of the eye, while the opposite imperfection (hypermetropy) results from too short an axis. He has found between a myopic and a hypermetropic eye a difference of as much as fourteen millimetres in the length of the optic axis. M. Dor carefully distinguishes the effect of age on the

accommodation and refraction of the eye. In a subsequent memoir he has reviewed the opinions of M. Donders on astigmatism or irregularities of refraction in the larger circles of the eye, an irregularity which always exists, more or less, but which in some cases may considerably alter the vision. The irregular curvature of the cornea is ordinarily the cause of it, but that of the crystalline also exists, and the result is that cylindrical glasses of different focal power may be eligibly employed. Numerous details are entered into respecting the effects of astigmatism and the proper means for ascertaining the cause, as well as supplying a remedy by a judicious choice of glasses.

The composite eyes of arthropods form the subject of a special memoir by M. Dor, printed in the *Bibliotheque Universelle* for December, 1861. In this the author, after reviewing the opinions propounded by anatomists, gives an account of direct experiments made by himself on the transmission of images through the cornea of sundry insects, and arrives at the conclusion that every facet of the composite eye is analogous to the single eye of one of the vertebrata—the cupuliform envelope being analogous to the retina.

M. Claparede read a memoir on the oligochetous worms of the environs of Geneva. These animals, having until now been little studied, have consequently presented a great number of new species and even new genera. Figures delineated by the author make us acquainted with some of them, but his chief object has been to describe the singular modifications of the reproductive apparatus. This apparatus, analogous to that of certain annelida (Pachydrilus) previously described by our author, shows that the excretory organ of the segments becomes now a vas deferens and an oviduct, and now a receptacle of the semen. The oligochetæ live in fresh water, but as marine animals always offer a wider field to the researches of zoologists, our learned secretary has not failed to visit, as often as circumstances would permit, the shores of the ocean. A sojourn in Normandy has enabled him to study the Turbellaria and the Tubularia, whose development and mode of reproduction offered him some remarkable peculiarities. As one of the fruits of this excursion, he has designed a series of plates relative to the embryology of marine worms, which were exhibited to this Society. Dr. W. Marcet communicated to us from London the result of some observations which he has been making on the gastric juice of the dog. When this juice is secreted under the influence of cartilaginous bones, it contains a peculiar substance analogous to the peptone of Lehmann.

BOTANY.

Professor Marcet presented an analysis of the labors of M. Daubeny on the absorption of different substances, particularly poisonous ones, by the roots of plants, (*Biblioth. Univ. Archiv. Sc.*, February, 1862.) M. Thury, who has been for some time occupied with a treatise on vegetable physiology, recapitulated the experiments of divers authors on the transpiration of vegetables; he has repeated many of them, and among other results has satisfied himself that the phenomenon, as had been asserted, continues when the plant is placed in water.

It has been said that the egret or tuft of the compositæ is often separated from the body of the grain, and hence does not favor the dissemination, as had been supposed. M. Thury, having observed on one of the summits of the Jura large quantities of the grains of the *Cirsium* transported thither by the winds, ascertained that three out of thirty still bore the seeds with them. The compiler of the present report, being in habits of correspondence with divers travellers, communicated interesting letters from M. Sagot on the flora of Guiana, and of M. Welwitsch on the vegetation of the high country of Huilla, in the interior of southwestern Africa, (*Biblioth. Univ.*, July, 1861.) M. Duby gave an account of a memoir published by M. Bail on an hypoxylon which propagates its mycelium in the interior of aged trunks of trees.

M. J. Müller, of the canton of Aargau, presented an important memoir on the classification of lichens and on the species of the environs of Geneva. The total number of our lichens exceeds 500, which the author enumerates and gives the description of 20 new species. The principles of classification adopted by M. Müller are analyzed and developed in the forthcoming volume of our Memoirs. There can be no doubt that it will attract the attention of lichenographers, seeing that this branch of botany has fallen into singular confusion resulting from the multiplicity of new characters and novel ideas introduced into it.

Local botany and descriptive botany scarcely adapt themselves to public lectures; our sessions, therefore, give but an inexact idea of the researches of several of our associates in these two departments of the science. I shall content myself with mentioning that M. Reuter has published in the course of the year a second and much augmented edition of his *Catalogue of vascular plants of the environs of Geneva*, and M. Boissier an important monograph of the tribe of the *Euphorbiæ*, in the fifteenth volume of the Prodromus.

De Candolle Prize.—The quinquennial prize, founded by A. Pyramus de Candolle, for the best monograph of a genus or family of plants, has this year claimed the attention of the Society. Contrary to what has heretofore occurred, our own countrymen have not entered into the competition for it. Two memoirs have been received. One of these was from M. de Bunge, professor in the University of Dorpat, on the Anabaseæ, a tribe of the Salsolaceæ or Chenopodiaceæ. The author having explored the shores of the Caspian sea and the interior of Persia, has there discovered several species of these plants, and the affluence of the materials at his disposal has enabled him to compile a very complete monograph. He has enlarged the number of genera from twelve to sixteen, while that of species continues to be sixty, notwithstanding the assignment of fourteen new species. Four charts or tables added to the text indicate the geographical distribution and relative affinities of the genera. The descriptions, given in Latin, are greatly developed. The second memoir was from M. Bäyer, inspector in chief of the Austrian railroads. The author treats of the genus *Tilia*, in relation principally to the numerous modifications of specific forms. He has tried a new system of notation, by letters, to express the varieties and subvarieties, each letter indicating a certain modification of character. This original idea deserves attention under a practical point of view. It is difficult to determine whether it would adapt itself to more numerous genera and to modifications of very different value which exist in certain groups. The jury which you nominated to decide between the competitors was struck with the value of the two memoirs submitted to them, both seeming to deserve high approbation; yet as there was but a single premium to award, it was considered that the monograph of M. de Bunge possessed superior claims on account of the number of species studied, the difficulty of the analysis, and variety of questions examined. The Imperial Academy of St. Petersburg, to which the author belongs, has equally appreciated the importance of his work, and directed it to be inserted in its memoirs.

"PERSONNEL" OF THE SOCIETY.

Since the last report our regrets have followed to the grave three of our colleagues: MM. Elie Ritter, L. A. Necker de Saussure, and Louis T. F. Colladon. The first, one of the most active and efficient members of the Society, fulfilled, for sixteen years, the exacting functions of secretary. When he resigned that office it was not with a view to withdraw himself from the claims of science and of our association; on the contrary, he ceased not to furnish his regular tribute of memoirs and communications on the most varied subjects. His treatise on the mathematical theory of music, of which I have previously spoken, but a little preceded his death, which occurred March 17, 1862, in the

61st year of his age. Devoted by choice to the career of instruction, he had been regent of the college in the department of mathematics, and was principal of the seminary for young females since 1855. He was thus led to the publication of elementary works, which have passed through several editions; but his views were always raised to the higher regions of science, and the list of his works furnishes abundant proof that he maintained himself on no inferior level in mathematics as well as astronomy and physics.

The two others whom I have named were *emeriti* of the Society. Louis A. Necker de Saussure, born April 10, 1786, felt the influence though he did not enjoy the personal instructions of his distinguished grandfather, Horace-Benedict de Saussure. The school of that great geologist had already evinced its influence in the exact experiments of Theodore the uncle, and in the judicious observations, though different in nature, of Mme. Necker de Saussure, mother of the subject of this notice. Nor did he himself delay in giving proof of the hereditary turn for science, his first publication, on the migration of birds, having been prepared at the age of 19 years. Still later he communicated, for the memoirs of our Society, a highly interesting enumeration of the birds of the environs of Geneva, a paper filled with well-observed facts, and attractive even to readers unacquainted with ornithology. After completing his studies at Edinburg, in 1807, a period at which he could scarcely escape the influences of the conflict which then prevailed between the partisans of Hutton and Werner, he travelled in Scotland as far as the Hebrides, and formed a taste for the wild scenery and hospitable society of that country which evinced its force at a much later period of his life. On his return to Geneva, he gave to the public in three volumes an account of his excursion to the Hebrides, which were then but little visited, and while availing himself of the great variety of objects to add a more popular charm to his work, fails not to give proofs of a spirit kindred to that of the author of the *Voyage dans les Alpes*. In his memoir on the Valley of Valorsine, published in 1828, which I regard as one of the best of his works, he still occupies the same field of study and of ideas with his celebrated ancestor, while in the later work *Etudes Geologiques dans les Alpes*, (1841,) he rather attaches himself, by the nature of his observations, to the modern school of Constant Prevost and Sir Charles Lyell. In this latter work, which was the first of the numerous series published by our cotemporaries on the more modern stratifications of our valley, the environs of Geneva are investigated, in view of the influence of existing causes, with especial care. Sharing the prevalent enthusiasm at the period of the Restoration, Louis Necker, for a while, bore arms; but the tendencies subsequently developed had separated him, since 1832, from all political affairs. In 1810 he had been appointed professor of geology and mineralogy in the Academy of Geneva, and made his zeal particularly conspicuous in the administration of the Museum, to which, in conjunction with MM. de Candolle, Deluc and Mayor, he contributed a series of lessons in zoology, for the benefit of that rising establishment. The best of his instructions, however, were his conversation and example, while traversing the mountains with a company of pupils. In 1823 De Candolle and he conducted an excursion for the purpose of study into the Chablais, and if the twelve young men who had the advantage of following them did not become naturalists or geologists, it was assuredly not the fault of their professors. On these occasions Necker possessed a gaiety truly inspiriting; nor were his accuracy of vision and method of observing less noticeable. After returning from Scotland, he had traversed the interior of France, a part of Italy, the western and also the eastern Alps, having made excursions of great interest into Styria and Carniola, his explorations on the banks of Lake Leman were incessant. With a view to the recovery of his somewhat exhausted strength, he again passed, in 1839, into Scotland, and the equal and humid climate of that country being found to agree with a too susceptible nervous system, led him to fix his residence

definitively in the remote island of Skye, on the western coasts of the kingdom. This man of cultivated mind and social qualities, cherished by a large circle of relatives and friends, deeply attached to Geneva, and after Geneva preferring Edinburg, where he had been happy in his youth, passed the last twenty years of his life in a profound solitude at Portree, a small hamlet of fishermen, on an island covered with mists and half desert! His health and temperament accommodated themselves to the situation. He still indulged his studious tastes. He observed whatever can be observed at Skye; the barometer, the thermometer, the opening of the scanty flowers, the arrival of migratory birds. Thanks to his meteorological observations, he could foretell tempests, and the humble inhabitants of Portree, whose lives are often exposed at sea, consulted this learned stranger with the white beard with a respect not unmingled, we may suppose, with some superstitious misgivings. Walter Scott would have certainly made him the hero of some romance.

Necker died at Portree, November 20, 1861. He had long before presented his collections in natural history to the Museum of Geneva, and I am now charged, by his nephew, M. Theodore Necker, with the acceptable duty of offering to this Society the notes taken during the last years of his life in Scotland, in order that examination may be made whether they contain anything from which advantage may be derived to science.

The third of our number whom we have lost, Louis T. F. Colladon, was born at Geneva, August 25, 1792. Having proceeded, after the usual preliminary education, to Montpellier, he was there kindly received by A. Pyramus de Candolle, who admitted him into a small party of special pupils, destined by the professor to the cultivation of botany. Under this influence and instruction, the proficiency of Colladon was evinced by the production of an esteemed monograph on the genus *Cassia*. Having graduated in medicine, he repaired to Paris, where his success in practice was satisfactory. That he carried both zeal and conscientiousness into his profession was shown by the courage and humanity with which, after having already retired from practice, he devoted himself to the care of the sick during the fearful cholera of 1832. Our colleague published an account of a descent into the sea in a diving-bell, (1816,) and a treatise, translated from the English, on deformities of the spinal column and the means of remedy, (1826.)

Among associates at large, *(associés libres,)* we have to deplore the recent loss of M. Charles Pictet, a young magistrate who would have rendered good service to his country. A distinguished talent for design often enabled his brother, M. Pictet de la Rive, to employ the resources of his pencil for objects of natural history.

The Society has recruited its ranks with two regular members, M. Alois Humbert, keeper of the Museum of Natural History, and M. Müller, author of several works on botany. At this time we number 36 regular members, 3 emeriti, 61 honoraries, and 35 associates at large. Our sessions have been well attended; the collection of memoirs increases each year by half a volume; scientific zeal seems in nowise diminished; we can therefore, I think, felicitate ourselves on the progress of the Society, and with satisfaction observe its entrance upon a new year.

ON THE CRANIA HELVETICA.

BY FREDERICK TROYON.

To the Secretary of the Smithsonian Institution:

SIR: I take the earliest opportunity of acknowledging, with many thanks, the receipt of the publications which you have been so kind as to send me on the part of the Smithsonian Institution, namely:

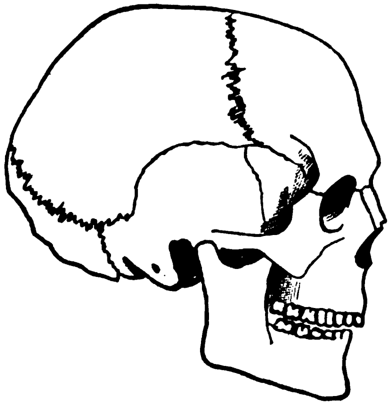
Smithsonian Report, 1862.
Ancient Mining on the Shores of Lake Superior.
Dictionary of the Chinook Jargon.
Comparative Vocabulary.
Instructions relative to Ethnology and Philology.
Annual Report of the Museum of Comparative Zoölogy.
Proceedings of the American Philosophical Society, Nos. 69, 70.

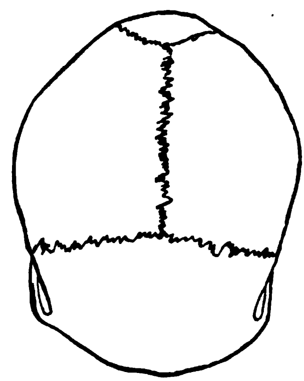
I propose soon to send you the new edition of my work on Lacustrian habitations, hoping that the copies which I offer will be accepted, not as a requital for your favors, but as a slight testimonial of my thanks and my high esteem.

I know not whether you already possess the work which MM. Rutimeyer and His, professors of anatomy at Basle, have recently published on the *Crania Helvetica*. However this may be, I am persuaded that you will not regard as inopportune a few observations on the subject of that interesting publication.

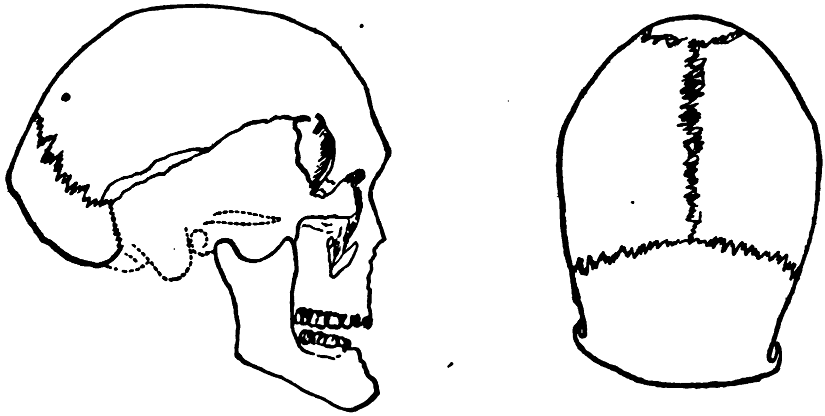
The authors distinguish, in reference as well to ancient as modern times, four different types of human skulls, which they designate from the names of the localities where the best specimens have occurred, in order not to prejudge results or historic questions. These types are those of Sion, in the Valais; of Hohberg, in the canton of Soleure; of Bel-Air, in the canton of Vaud; and of Disentis, in the Grisons. The following are the figures of these four types reduced to the fourth of their natural size:

I.—SION TYPE.

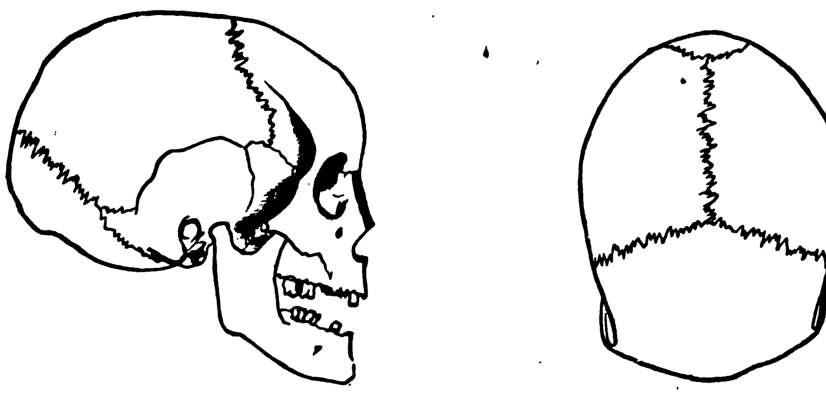




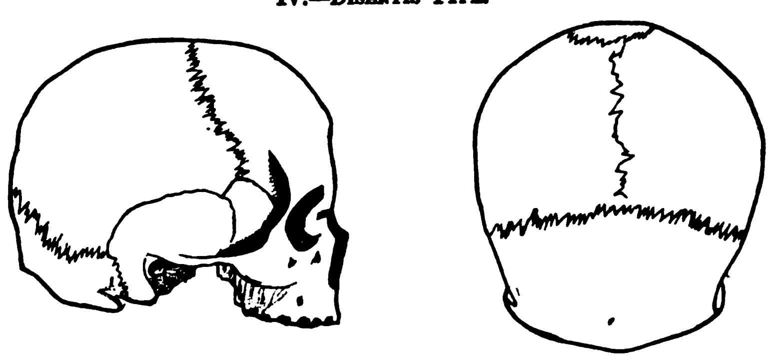
II.—Hohberg Type.



IV.—Bel-air Type.



IV.—Disentis Type.

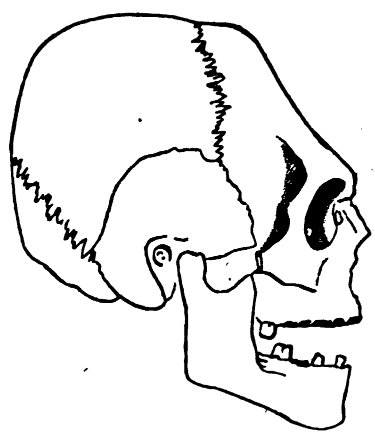


The basis upon which these different types are founded must be sought in the *Crania Helvetica*. I will only add that it is to the age of iron in Switzerland, i. e., to the five or six centuries which preceded the Christian era, the Sion type (I) is to be referred. The Hohberg type (II) answers to the Roman period, or

the first four centuries of our era. The type Bel-Air (III) only makes its appearance in the tombs of the Merovingian period; and the type Disentis, (IV,) which is the most widely spread in the Switzerland of modern times, was likewise that of the age of stone in ancient Helvetia. There is a very similar type found in the most ancient tombs of the Scandinavian countries; but according to a late communication which I have received from Baron Von Duben, professor at Stockholm, the Sion type also existed in Sweden in the age of stone.

A considerable number of the many skulls, whose figures are given by MM. His and Rutimeyer, form part of my own collection, and of these I have derived some very remarkable specimens from an extensive cemetery, situated at my country seat of Bel-Air, near Lausanne, which I have been exploring for several years. It was in this cemetery that I found the annexed skull, (V,) the only one of its kind, and in some respects like those of the ancient Peruvians; but this having been already published on more than one occasion, I shall abstain here from any comments in regard to it, in order to draw your attention to an observation of a more general nature.

V.



The tombs of Bel-Air, superposed in three successive layers underground, pertain to the period which elapsed from the fall of Rome to Charlemagne. I have explored since 1838, in the canton de Vaud, a great number of cemeteries of the same period, and notwithstanding the variety of skulls that are found of the same epoch, I observe that the prevailing type in these sepulchres is of an elongated form, whose anterior development is in general very slight. This observation is of interest in view of the fact that these tombs contain the remains of the true ancestors of the present population of the canton de Vaud, and that the general form of the skulls of this country presents, in modern times, less posterior development and a more rounded outline—a form, therefore, more advantageous in an intellectual point of view. We have here, then, a population in which we may remark that, notwithstanding the persistence of certain types, civilization has had for its result a sensible modification of the encephalon in an ascending scale, a direction in which progression is always much slower than in the opposite one of degradation.

Permit me, sir, with this remark, to renew to you the expression of my most distinguished consideration.

FRED. TROYON.

LAUSANNE, *November* 10, 1864.

EXPERIMENTAL AND THEORETICAL RESEARCHES

ON

THE FIGURES OF EQUILIBRIUM OF A LIQUID MASS

WITHDRAWN FROM THE ACTION OF GRAVITY, &c.

BY J. PLATEAU, PROFESSOR IN THE UNIVERSITY OF GHENT.

TRANSLATED FOR THE SMITHSONIAN INSTITUTION FROM THE MEMOIRS OF THE ROYAL ACADEMY OF BRUSSELS.

SECOND SERIES.

(CONTINUED FROM PAGE 285, SMITHSONIAN REPORT FOR 1863.)

Application of the properties of liquid cylinders: theory of the constitution of liquid veins emitted from circular apertures.

69. Let us now pass to the application which we have announced of most of the foregoing facts and laws.

Let us consider a liquid vein flowing freely by the action of gravity from a circular orifice perforating the thin wall of the horizontal bottom of a vessel. The molecules of the liquid within the vessel, which flow from all sides towards the orifice, as we know, still retain, immediately after their exit, directions which are oblique to the plane of this orifice; whence there is produced a rapid constriction of the vein, commencing at the orifice and extending as far as a horizontal section, which has been improperly denominated the contracted section. When the molecules have arrived at this section, which is very near the orifice, they all tend to assume a common vertical direction, with a velocity corresponding to the height of the liquid in the vessel; and they are, moreover, urged in this direction by their individual gravity. Hence, supposing the orifice to be circular, the vein commencing at the contracted section tends to form an almost perfect cylinder, of any length; but this form is modified, as we now know, by the acceleration which gravity imparts to the velocity of the liquid, and the diameter of the vein, instead of being everywhere the same, decreases more or less in proportion as we recede from the contracted section.

If the causes which we have detailed were alone in action, the vein would appear simply more and more attenuated in proportion as it is considered more distant from the contracted section, without losing either its limpidity or its continuity. But it results from our experiments, that a liquid figure of this kind, the form of which approximates to that of a very elongated cylinder, must become transformed into a series of isolated spheres, the centres of which are arranged upon the axis of the figure. In fact, we have here a liquid submitted to the action of gravity; but it is evident that during the free descent of a liquid, gravity no longer presents any obstacle to the play of the molecular attractions, and that the latter must then exert the same configuring actions upon the mass as if this mass were free from gravity and in a state of rest; this is the manner in which, for instance, drops of rain, during their fall, acquire the spherical form. But, for the preceding conclusion to be perfectly

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ERRATUM.—At page 207 of the Smithsonian Report for 1863, sixth line from the bottom, for "less than an inch in a *year*," read "less than an inch in a *day*." J. H.

rigorous, it would be requisite for all parts of the mass to be actuated by the same velocity, which is not the case with the vein; we can, however, understand, that, although this difference may be capable of producing some modifications in the phenomenon, it cannot prevent its production.

The liquid of the vein, therefore, during its movement must necessarily gradually form a series of isolated spheres. But as this liquid is constantly being renewed, the phenomenon of transformation must also continue to be renewed. In the second place, as each portion of the liquid begins to be subjected to the configuring forces as soon as it forms a part of the imperfect cylinder which the vein tends to form, *i. e.*, from the moment at which it passes the contracted section, and subsequently remains during its course under the continued action of these forces, it is evident that each of the *divisions* of the vein must begin to be formed at the contracted section and to descend, conveyed by the movement of transference of the liquid, modifying itself by degrees so as to arrive at the state of an isolated sphere. Hence it follows that, at any given instant, the divisions of the vein must exist in a more advanced phase of transformation in proportion as they are considered at a greater distance from the contracted section, at least as far as that at which the transformation into spheres is completely effected. From the orifice to the distance where the separation of the masses occurs, the vein must evidently be continuous; but at a greater distance, the portions of liquid which pass must be isolated from each other.

If, then, the movements of the liquid, both that of translation and that of transformation, were sufficiently slow to allow of our following them with our eyes, the vein would appear to be formed of two distinct parts, the one superior and continuous, the other inferior and discontinuous. The surface of the former would present a series of dilatations and constrictions, which would descend with the liquid, becoming constantly renewed after passing the contracted section, and which, although very feebly indicated at their origin near this section, would become more and more marked during their movement of transference, the dilatations becoming more prominent and the constrictions narrower: these divisions of the vein arriving one after the other, in their greatest development, at the lower extremity of the continuous part, would be seen to become detached from it, and immediately to complete their assumption of the spherical form. Moreover, the separation of each of these masses would necessarily be preceded by the formation of a line which would resolve itself into spherules of different diameters, so that each isolated sphere would be succeeded by similar spherules. The discontinuous part of the vein would then be seen to consist of isolated spheres of the same size, and of equal spherules arranged in the intervals of the former, both of them being conveyed by the movement of translation, and being unceasingly renewed at the extremity of the continuous part.

Now Savart's beautiful investigations* have taught us that this is, in fact, the real constitution of the vein; except that under ordinary circumstances an extraneous cause, which was also pointed out by Savart, more or less modifies the form of the divisions of the continuous part, and alters the sphericity of the isolated masses composing the discontinuous part; but Savart has given the means of excluding this influence, of which we shall speak hereafter.

70. Now as the movement of transference is too rapid to allow of the phenomena which are produced in the vein being recognised by direct observation, certain peculiar appearances ought to be the result of this. We must remember here, that when a liquid cylinder becomes resolved into spheres, the rapidity with which the transformation takes place is accelerated, and consequently at the commencement is extremely small. In consequence, then, of this original minuteness and of the velocity of the movement of transference in the vein,

* *Annales de Chimie et de Physique*, Août 1833.

the effects of the gradual transformation cannot begin to become obvious until a greater or less distance from the contracted section has been attained. Up to this distance the rapid passage of the dilatations and constrictions before the eye cannot give rise to any effect visible to the simple sight; so that this portion of the vein will appear in the form which it would affect if it had no tendency to become divided. Beyond this distance the dilatations will begin to acquire considerable development; the vein will appear to continue enlarging until another distance has been attained beyond which the diameter will appear constant. Such is, in fact, as the observations of Savart have shown, the form presented to direct observation by a vein withdrawn from the influence of any disturbing cause.

Lastly, we know that from the orifice to the point at which it appears to begin to enlarge, the vein is seen to be limpid, whilst further on it appears more or less turbid; and Savart has perfectly explained these two different aspects, as also some other curious appearances which the troubled part presents, by attributing the limpidity of the upper portion to the slight development of dilatations and constrictions which are propagated in it; and the turbidity, as also the other appearances of the remainder of the vein, to the rapid passage before the eye, at first of the dilatations and constrictions which have become more marked, then, lower down, of the isolated spheres and the interposed spherules. We must refer for the details to the memoir quoted above.

71. But we may go further: two consequences spring directly from our explanation of the constitution of the vein. In the first place, as the divisions become transformed during their descent, it is clear that the space traversed by a division during the time it is effecting a given part of its transformation will be as much greater as it descends more rapidly, or, in other words, as the charge, *i. e.*, the height of the liquid in the vessel, is more considerable; whence it follows clearly that, the orifice being the same, the length of the continuous part of the vein must increase with the charge. Now this has been confirmed by Savart's observations. In the second place, since the transformation of a cylinder is slower in proportion to the size of its diameter, the time which a division of the vein will occupy in effecting any one and the same part of its transformation will be as much longer as the vein is thicker; whence it follows, that if the rapidity of the flow does not change, the space which the division will traverse during this time will be as much greater as the diameter of the orifice is greater; consequently, for the same charge, the length of the continuous part must increase with the diameter of the orifice, and this is also verified by the observations detailed in the memoir quoted.

With regard to the laws which regulate these variations in the length of the continuous part, Savart deduces from his observations, which were made by employing veins of water, that for the same orifice this length is nearly proportional to the square root of the charge, and that for the same charge it is nearly in proportion to the diameter of the orifice.

Let us now examine whether these two laws also emanate from our explanation.

72. Imagine for a moment that gravity ceases to act upon the liquid as soon as the latter passes the contracted section. Then, commencing at this section, the rapidity of translation will simply be that which is due to the charge, and the value of which, as we know, is $\sqrt{2gh}$, g denoting gravity and h the charge. This velocity will be uniform; consequently, if the vein had no tendency to divide, it would remain exactly cylindrical throughout any extent, (§ 69.) Now, all parts of the liquid being actuated by the same velocity of transference, this common movement cannot exert any influence upon the effect of the configuring actions; so that, for instance, the gradual modifications which each of the constrictions undergoes, and the time which it takes in their accomplishment, will be independent of the rapidity of transference.

This admitted, let us consider the infinitely thin section which constitutes the neck of a constriction at the moment at which it quits the contracted section. This section will descend with a constant velocity, and at the same time its diameter will continually diminish until the constriction to which it belongs becomes transformed into a line, and then the section in question will occupy the middle of this line; the line will become disunited, to be converted into spherules. As we have shown above, the time employed in the accomplishment of these phenomena, and during which the liquid section we have considered has traversed the distance comprised between the contracted section and the place which the middle of the line occupies at the precise instant of rupture, is independent of the velocity of transference; consequently, if the diameter of the orifice does not change, this time will be constant, whatever may be the charge. Now, when the movement is uniform, the space traversed during a determinate time being in proportion to the velocity, the above distance will be in proportion to $\sqrt{2gh}$, consequently to $\sqrt{h}$. As we shall frequently have occasion to make use of this distance, we shall represent it, for the sake of brevity, by D.

Now, it is easily understood that in our vein the length of the continuous part does not differ sensibly from the distance D. In fact, the continuous part terminates at the exact place at which, in each line, the most elevated of the points of rupture of the latter is produced; for at the instant at which the rupture takes place, the phases of transformation of all that portion which is above the unit in question are less advanced, (§ 69,) and therefore it still possesses continuity, whilst all that below this point is necessarily already discontinuous. Thus, on the one hand, the continuous part of the vein commences at the orifice and terminates at the place at which the most elevated point of rupture of each filament is produced; and, on the other hand, the distance D commences at the contracted section, and terminates at the point corresponding to the middle of the length of each of the lines at the instant of their rupture. The continuous part then takes its origin rather higher up, but also terminates a little above the distance D; the difference in the origins of these two magnitudes and that of their terminations must, consequently, partially compensate each other; and as these differences are both very minute, the excess of one over the other will, *a fortiori*, be very small, so that the two magnitudes to which they refer may, as I have stated, be regarded, without any sensible error, as equal to each other.* In virtue of this equality, the length of the continuous part of the vein which we are considering will then apparently follow the same law as the distance D, *i. e.*, it will be very nearly proportional to $\sqrt{h}$.

Thus, in the imaginary case of uniform velocity of transference, we again recognize the first of the laws given by Savart. Now, it is clear that in a real vein the velocity will deviate from uniformity so much the less as the charge is greater; whence we may infer that, for sufficiently great charges, the length of the continuous part of the real vein must still exactly follow this law. We shall, moreover, demonstrate this in a rigorous manner.

73. Let us, then, take the real case, *i. e.*, let us consider a vein submitted to the action of gravity, in which, consequently, the movement of transference is accelerated. Then the velocity possessed, after any time t, by a horizontal section of the liquid conveyed by the movement of transference, will have for its value $\sqrt{2gh}+gt$, the first term representing the portion of the velocity due to the charge, the second the portion due to the action of gravity upon the vein, and t being reckoned from the moment at which the liquid section passes the contracted section. It must be borne in mind that, in virtue of the accele-

* We shall recur to this point, and shall then establish it more clearly.

ration of the velocity, the vein, if it did not become divided, would continue to become indefinitely thinner from above downwards, (§ 69.)

This admitted, let us imagine that another vein of the same liquid, placed under the imaginary condition of the preceding paragraph, flows off with the same charge from another orifice of the same diameter, in the same time as the true vein in question. Let θ denote the time occupied by this second vein in traversing the distance which we have denoted by D, *i. e.*, that which is comprised between the instant at which the liquid section that constitutes the neck of a constriction passes to the contracted section, and the instant of the rupture of the line into which this constriction becomes transformed. In the expression of the velocity relative to the first vein, let $t=\theta$, which gives for this velocity, after the time θ, the value $\sqrt{2gh}+g\theta$; in other words, let us consider the velocity of a liquid section belonging to the true vein, after the time necessary for a section belonging to the imaginary vein to have traversed the distance D. According to what we have seen in the preceding section, if the orifice remains the same, this time is constant, whatever the charge may be; so that in the above expression the term $g\theta$ remains invariable when h is made to vary. Hence, whatever may be the value of θ, we may suppose the charge h to be sufficiently large for the term $\sqrt{2gh}$ to be very great in proportion to the term $g\theta$, and that the latter consequently may be neglected without any sensible error. In the case of a value of h which will realize this condition, and, *a fortiori*, in the case of all still greater values, the velocity of a section of the true vein during the time θ may be regarded as constant, and equal to that of a section of the imaginary vein; so that throughout the entire space traversed by the first during this time, commencing at the contracted section, the real vein, if it did not become divided, would preserve exactly the same diameter, and might be regarded as identical with the imaginary vein, also assumed to be free from divisions.

Now, it necessarily follows, from this approximative identity, that during the time θ the same will apparently occur in like manner in both veins; consequently the time θ will be very nearly that which, in the true vein, the liquid section, corresponding to the neck of a constriction, would employ in accomplishing the modifications which we have considered, and the space which it will traverse during these modifications may be regarded as equal to the distance D relative to the imaginary vein.

Now, as the continuous part of the true vein terminates a little below this space, and is consequently included in the same portion of the vein, it follows, from the above approximative identity, that this continuous part will be exactly equal in length to that of the imaginary vein, and therefore, commencing with the least of the charges considered above, the lengths of the continuous parts of both veins must be very nearly governed by the same law.

We arrive then, lastly, at this conclusion, that for the same orifice, and commencing with a low but sufficient charge, the length of the continuous part of the true vein must be in proportion to the square root of the charge.

In accordance with the preceding demonstration, the low charge in question is that at which the movement of transference of the liquid begins to remain apparently uniform in all that portion of the true vein which is comprised between the contracted section and the point occupied by the middle of each line at the instant of rupture; but as the extremity of the continuous part is very little distant from this point, (§ 72,) we may neglect the small difference, and say simply that the low charge in question is that which begins to render the movement of transference of the liquid exactly uniform as far as the extremity of the continuous part of the vein.

Thus, under the condition of a low charge sufficient to produce this approximative uniformity, which condition is always realizable, the law indicated by Savart as establishing the relation between the length of the continuous part

and the charge necessarily follows from the properties of liquid cylinders. To discover whether this law is also true when weaker charges are employed, we must start from other considerations; but it is evident, so far, that if in the latter case the law is different, it must at least necessarily converge towards the proportionality in question, in proportion as the charge increases.

We must remark here, that in the case of a given liquid, the charge with which the vein begins to exist under the condition which we have determined must be as much less as the diameter of the orifice is smaller. In fact, since, all other things being equal, the transformation of a liquid cylinder occurs with a rapidity proportionate to the diminution in size of the diameter of the cylinder, it follows that the value of θ will diminish with the value of the orifice; and therefore the smaller the latter is, so much the less will the value of h become to allow of the term $g\theta$ in the expression $\sqrt{2gh}+g\theta$, placed at the commencement of this section, being neglected in comparison with the term $\sqrt{2gh}$, and consequently for the vein to exist under the condition in question.

Moreover, as the time θ varies with the nature of the liquid, the same will necessarily apply to the charge under consideration.

74. Let us now investigate the second law, namely, that which establishes the approximative proportion of the length of the continuous part of the vein and the diameter of the orifice, when the charge remains the same.

Let us resume, for an instant, the imaginary case of an absolutely uniform movement of transference. The vein, leaving its divisions out of consideration, will then constitute a true cylinder commencing at the contracted section, (§ 72,) which cylinder will be formed in the air, and the entire convex surface of which will be free; moreover, as the movement of transference of the liquid does not exert any influence upon the effect of the configuring forces, (§ 72,) and as there is no extraneous cause tending to modify the length of the divisions, the latter will necessarily assume their normal length. It is evident, therefore, that excepting that the formation of its divisions is not simultaneous, (§ 69,) our imaginary vein will exist under exactly the same circumstances as the cylinders to which the laws recapitulated in section 68 refer; consequently, if we consider in particular one of the constrictions of this vein, it must pass through the same forms, and accomplish its modifications in the same time, as any one of the constrictions which would result from the transformation of a cylinder of the same diameter as the vein, formed of the same liquid, and placed under the conditions in question.

Now, in the case of a cylinder of mercury, the time comprised between the origin of the transformation and the instant of the rupture of the lines is, in accordance with one of our laws, exactly or apparently in proportion to the diameter of the cylinder; and it is clear that this law is equally applicable to any one of the constrictions in particular, or even simply to its neck, as to the entire figure. If, then, we suppose our imaginary vein to be formed of mercury, the time which the neck of each of its constrictions will occupy in arriving at the instant of the rupture of the line will be exactly or apparently in proportion to the diameter which the vein would possess if the divisions in it were not formed, *i. e.*, to that of the contracted section. Now, as the cylindrical form of the vein, supposed to exist without divisions, only begins at the contracted section, it is only from this part that the configuring actions arising from the instability of this cylindrical form commence. We must, therefore, admit that the liquid section, which constitutes the neck of a constriction, does not begin to undergo the modifications which result from the transformation until the instant at which it passes the contracted section; thus the interval under consideration commences at this very instant.

But this interval, comprised between the instant at which the liquid section of which the neck of a constriction is formed passes the contracted section, and the instant of the rupture of the line into which this constriction becomes

converted, is that which we have designated by θ, and in which the liquid section traverses the distance D; in our imaginary vein of mercury, the time θ will therefore be in proportion to the diameter of the contracted section.

Now, we know that in a liquid vein, the diameter of the contracted section may be regarded as proportional to that of the orifice, when the latter exceeds 6 millimetres, and that above this limit the proportionality does not alter very appreciably except when the diameter of the orifice becomes less than a millimetre.* Moreover, as this alteration is attributed to the influence which the thickness of the edges of the orifice, although very slight, exerts, it is probable that it may be rendered still less by employing, as Savart has done, orifices expanded outwardly, and which may be shaped so that their edges may be very sharp. Thus, with properly made orifices, we may undoubtedly admit, without appreciable error, that commencing with a diameter equal at most to a millimetre, the diameter of the contracted section is proportional to that of the orifice.

Hence, as the length of the continuous part of our imaginary vein is in proportion to the diameter of the contracted section, it will also be in proportion to the diameter of the orifice, at least starting from a low value of the latter, which must not be much less than a millimetre.

We have only considered the case of mercury; but the principle with which we set out, *i. e.*, the proportionality between the partial duration of the transformation of a cylinder and the diameter of the latter, very probably applies also, as we are already aware, to all other very slightly viscid liquids; consequently, in the case of any of the latter liquids, it is very probable that the length of the continuous part of the imaginary vein will also be in proportion to the diameter of the orifice. The law may also be true in regard to all liquids; but it may be the case that this general application does not hold good.

If we now pass from the imaginary to the true vein, we have only to suppose that the value of the constant charge is sufficiently considerable to allow of the condition assumed in the preceding section being satisfied throughout the entire extent which we assign to the variations in the diameter of the orifice; so that, for each of the values given to this diameter, the continuous part of the true vein is apparently of the same length as that of the corresponding imaginary vein. The law which regulates this length may then be regarded as the same in both kinds of veins. In accordance with the two remarks terminating the preceding section, it is evident that if the common charge fulfils the condition in question with regard to the greater value assigned to the diameter of the orifice, it will, *a fortiori*, fulfil it with regard to all the others.

We are, therefore, led to the following definitive conclusion:

In the case of mercury, and very probably also in that of all other very slightly viscid liquids, such as water, if for the same charge increasing values are given to the diameter of the orifice, from a value slightly less than a millimetre to some other determinate value, and if the common charge be sufficiently great, the length of the continuous part of the vein will be proportionate to the diameter of the orifice.

This conclusion is, perhaps, true in the case of any liquid whatsoever; but the elements for deciding this question are wanting.

Thus, with the restrictions contained in the above enunciation, the second law given by Savart results necessarily from the properties of liquid cylinders; and it is also evident that if, in the case of a common inconsiderable charge,

* In fact, the results obtained by Hachette show (*Ann. de Chim. et de Phys.*, t. iii, p. 78) that when the diameter of the orifice is equal to or greater than 10 millimetres, the mean proportion of the diameter of the contracted section to that of the orifice is 0.78; that in passing from 10 millimetres to 1 millimetre, the proportion only increases 0.83; and lastly, when the diameter is equal to 0.55 millimetre, the proportion becomes 0.88.

the law becomes modified, it must approximate towards that of Savart in proportion as the value given to this charge is greater.

75. We said (note to § 72) that we should return to the closely approximative principle of equality between the length of the continuous part of an imaginary vein and the corresponding distance D, in order to establish this principle more clearly; we shall now do this.

Let L be the length of the continuous part, and C the portion common to this length and the distance D; let also s be the interval between the origins of the lengths L and D, *i. e.*, the small distance comprised between the orifice and the contracted section; and, lastly, let i be the interval between the terminations of these same lengths, *i e.*, the distance comprised between the uppermost point of the rupture of the line and the middle of this line; we shall then have

$$L=C+s,$$
$$D=C+i;$$

consequently

$$L-D=s-i;$$

whence

$$\frac{L}{D}=1+\frac{s-i}{D} \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots (1.)$$

Let us now first approximatively value the quantity i in the case of some particular liquid, and let us again take mercury. After what was shown at the commencement of the preceding section, the length of the divisions of an imaginary vein is equal to the normal length of those of a cylinder of the same diameter and of the same liquid which would be formed in the air, and the entire convex surface of which is free; now in the case of mercury, we know that the proportion of this normal length to the diameter of the cylinder must be less than 4; consequently, in our imaginary vein of mercury, the proportion of the length of the divisions to the diameter of the contracted section will also be less than 4; but in our state of ignorance of the exact value of this proportion, we will first suppose it to be equal to the above number. If we then denote the diameter of the contracted section by k, the diameter of the isolated spheres composing the discontinuous part of the vein will be (§ 60) equal to $1.82.k$, and the length of the interval between two successive spheres will be $2.18.k$. But the line into which a constriction is converted is necessarily shorter than this interval; for so long as the rupture does not take place, the two masses united together by the filament must still be slightly elongated; and, moreover, each of them must present a slight elongation of the line, so as to be connected to the latter by concave curvatures. Judging from the comparison of the aspects presented immediately after the rupture of the line, and after the entire completion of the phenomena, by the figure resulting from the transformation of one of our short cylinders of oil, (see figs. 28 and 29,) I should estimate that for each of the two masses connected by a line, the elongation towards the latter *plus* the slight concave prolongation form about two-tenths of the diameter which these masses acquire after their transition to the state of spheres. To obtain the approximative value of the line belonging to our vein, we must therefore deduct from the interval $2.18.k$, four-tenths of the diameter $1.82.k$, which gives $1.45.k$. On the other hand, if we denote the diameter of the orifice by K, we have (note to the preceding section) very nearly $K=0.8.K$; whence it follows that the approximate value of the length of our line is equal to $1.45\times0.8.K=1.16\,K$. Lastly, the uppermost point of rupture of the line must be very near the upper extremity of the latter; if we suppose it to be at this extremity itself, the quantity i will be half the length of the line, and we shall consequently have

$$i=0.58.K.$$

Let us pass to the quantity s. We know that the distance between the orifice and the contracted section, although not entirely independent of the charge, always differs but little from the semi-diameter of the orifice, so that we should have very nearly $s=0.50.K$, and therefore

$$s-i=0.50.K-0.58.K=-0.08.K,$$

evidently a very slight difference.

We have assumed 4 as the value of the proportion of the length of the divisions of our vein to the diameter k; this value is undoubtedly too great; but as the exact value must necessarily exceed the limit of stability, which is itself more than 3, we may admit that this exact value is considerably more than the latter number. Suppose it, however, to be equal to this number 3; calculation will then give for the diameter of the isolated spheres the quantity $1.65.k$, and for the interval between two consecutive spheres the quantity $1.35.k$. Completing the operations with these data, in the same manner as above, we obtain as the final result

$$s-i=0.23.K,$$

also a very slight difference.

Now, as the true value of the difference $s-i$ must be comprised between the two limits which we have just found, *i. e.*, $-0.08.K$ and $+0.23.K$, and as we cannot ascertain either the one or the other, we shall obtain a sufficient approximation to this true value by taking the mean of the two above limits, which gives, lastly,

$$s-i=0.07.K \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots (2.)$$

Let the distance remain D. As this is traversed by a uniform movement during the time θ and with the velocity $\sqrt{2gh}$, we shall first have

$$D=\theta\sqrt{2gh}.$$

Now, as the time θ is equal (preceding section) to the partial duration of the transformation of a cylinder of the same diameter and of the same liquid as the vein, and which would be formed under the conditions of the results summed up in § 68, it follows, from one of the latter, that if the diameter of the contracted section of our imaginary vein of mercury were a centimetre, the time θ would be considerably more than 2 seconds; however, in order to place ourselves intentionally under unfavorable circumstances, let us suppose that, in the above case, the time in question were only equal to 2 seconds. But the time θ is proportionate to the diameter of the contracted section, (preceding section;) if, then, we take the second as the unit of time and the centimetre as the unit of length, we shall have for any value k of this diameter

$$\theta=2k;$$

and if we replace k by its approximative value $0.8.K$, it will become

$$\theta=1.6.K;$$

consequently

$$D=1{\cdot}6.K\sqrt{2gh}.$$

As we have taken the second and the centimetre as the units of time and length, g will be equal to 980.9; and this value being substituted in the above expression, it will finally become

$$D=70.87.K\sqrt{h}.$$

From this expression, and that of $s-i$ given by the formula (2,) we deduce

$$\frac{s-i}{D}=\frac{0.07}{70.87.\sqrt{h}}=0.001.\frac{1}{\sqrt{h}}.$$

Now, according to the equation (1) this quantity represents the error we commit in supposing $\frac{L}{D}=1$, or $L=d$; it is evident that this error is independent of the diameter of the orifice, but that it varies with the charge, and that it is

less in proportion as the strength of the charge is greater; it is also evident that for it not to be very small, an extremely small value must be given to the charge; for when the charge is too small, either the flow does not take place, or it ensues drop by drop, in both which cases the nature of the phenomenon is changed, and cannot be referred to the transformation of a cylinder. We shall therefore suppose that the value of the charge is 4 centimetres, for instance, which is certainly a small value, and which is slightly greater than the least of the values employed by Savart in his experiments. We shall then have

$$\frac{s-i}{D}=0.0005;$$

and transferring this value to the equation (1) we shall find

$$\frac{L}{D}=1+0.0005,$$

or rather

$$L-D=0.0005 \,.\, D.$$

Thus, according to this result, whatever the diameter of the orifice may be, with the feeble charge of 4 centimetres, the length of the continuous part of an imaginary vein of mercury only exceeds the distance D by a quantity equal to 6 ten-thousandths of the latter; so that, for instance, if the diameter of the orifice were such that the distance D were a metre, the length of the continuous part would only differ from it by half a millimetre; and in consequence of the very small value we have attributed to θ, even this probably exceeds the true difference. Lastly, if we pass from mercury to some other liquid, the difference between L and D, or rather the proportion of this difference to D, would necessarily vary in magnitude and direction with the nature of the liquid; but this proportion, as we have shown, is so small that we may safely admit that it will always be very small in regard to any other liquid.

76. Let us now go within the limit commencing with which the real vein may be compared, in its continuous part, to the corresponding imaginary vein, (§§ 73 and 74;) in other words, let us suppose the charge to be so inconsiderable, or the diameter of the orifice to be so great, that the movement of transference, in the extent of the continuous part of the real vein, is not perfectly uniform. The vein will also then tend to become thinner from above downwards, and this diminution in thickness will become visible upon the limpid portion. The question of the laws which under these circumstances must regulate the length of the continuous part is very complicated; we shall, however, attempt to elucidate it to a certain point.

Let us consider a division of the vein at the instant at which its upper extremity passes the contracted section. The two liquid sections between which the division in question is comprised separate from this position with different velocities; for, in the short path which the inferior section has traversed, its velocity is even slightly augmented by the action of gravity. Now, it follows from this excess of velocity and the acceleration of the motion, that the two sections will continue to separate from each other more and more in proportion as they descend; or, in other words, that the portion of the liquid included between them will gradually become elongated during its motion of transference. Consequently, if no other cause intervened, each of the divisions, conveyed by the accelerated velocity of the liquid, would gradually increase in length up to the instant of the rupture of the line, and would preserve a constant volume during its descent.

But there is a cause which acts in an opposite manner upon the divisions. If we imagine the divisions of the continuous part to be suddenly effaced, the small portion of the vein thus modified which replaces, at this instant, any given division, will be smaller in proportion as the division in question is more distant

from the contracted section. Consequently we may consider each of the divisions which at a determinate instant are arranged upon the entire length of the continuous part as arising respectively from the transformation of a different cylinder; and as the minute portion of the vein which replaces, in the above hypothesis, any given division would continue slightly diminishing in thickness from above downwards, we should exactly obtain the diameter of the corresponding cylinder by taking the mean diameter of this portion. Now, we know that for any liquid, the normal length of the divisions of a cylinder supposed to be formed in the air, and the entire convex surface of which is free, is in proportion to the diameter of this cylinder; consequently, if nothing opposed the action of the configuring forces upon the vein, the proportion of the length of a division to the above mean diameter corresponding to it would be the same for all the divisions; and as this mean diameter diminishes at each division from the top to the bottom of the continuous portion, it follows that the length of the divisions would continue to decrease in the same proportion. If, then, the cause with which we are engaged were alone in action, each division would gradually diminish in length and volume in proportion as it descended in the continuous portion. But then the divisions starting from the contracted section with the velocity of the liquid would necessarily follow in their movement of transference a different law. We shall show that this movement would be retarded, so that the liquid, which descends, on the contrary, with an accelerated velocity, must pass from one division to the other, and that the latter must simply constitute, upon the surface of the vein, a sort of undulation, which would be propagated according to a particular law.

Let us assume the hypothesis of the entirely free action of the configuring forces, and let us commence with the moment at which the section of the surface of the vein which constitutes the neck of a constriction passes to the contracted section. After a brief interval, another superficial section, corresponding to the next neck, will pass in its turn, and these two sections will include a division between them. After another interval of time equal to the first, another division will have passed to the contracted section; but the first will even then be shortened, so that its lower neck, in this second interval of time, will have traversed a less space than the first. For the same reason, the space traversed in a third interval of time equal to the two others will be still smaller, and so on afterwards. The movement of transference of the necks, and therefore that of the divisions which they include, two and two, will then constitute, as I have stated, a retarded movement.

Now, the two causes which we have mentioned, and which act concurrently upon the divisions, will necessarily combine their effects. Consequently the velocity of transference of the divisions will be intermediate between the accelerated velocity of the liquid and the retarded velocity which would result from the second cause alone; in the second place, the divisions will gradually diminish in volume during their descent along the continuous portion, but according to a less rapid law than would be the case under the isolated action of this second cause; lastly, the length of the divisions will follow a law intermediate between the gradual increase determined by the first cause and the decrease produced by the second.

77. We shall now investigate the manner in which these modifications in the volume, length, and velocity of the divisions are capable of exerting an influence upon the laws regulating the length of the continuous portion of the vein.

We must first draw attention to the fact that in our imaginary veins, where the movement of transference of the liquid is supposed to be uniform with all charges, the causes producing the above modifications do not exist; consequently the divisions must always descend with the same velocity as the liquid, without varying in either volume or length in the course of the continuous part. Moreover, we must recollect that after what has been detailed in §§ 72,

74 and 75, Savart's laws are already satisfied with regard to these veins commencing with very feeble charges; the first law in the case of any liquid whatever, and the second in the case of mercury, very probably also in that of any other very slightly viscid liquid, and perhaps even in that of all liquids.

Let us now recur to the true vein of the preceding section, and let us begin by examining the influence exerted by the diminution of the volume of its divisions. Since a cylinder, supposed to exist under the conditions of our laws and formed of a given liquid, becomes transformed with rapidity proportionate to the smallness of its diameter, it necessarily follows that as the volume of its divisions is smaller, the gradual diminution in the volume of the divisions of the vein tends to render the velocity of their transformation more accelerated than it would be in the imaginary vein of the same liquid if it flowed under the same charge, and from an orifice of the same diameter. Under the isolated influence of this modification of the volume, the time which the portion of the phenomenon corresponding to the course of the continuous portion requires would therefore be shorter; consequently the length of this portion would be less than in the imaginary vein. Now if the charge under consideration were replaced by a charge very nearly sufficient to annihilate the acceleration of the movement of transference of the liquid in the continuous part, this portion of the vein would then be equal in length to that of the corresponding imaginary vein, (§ 73;) therefore in passing from the first charge to the second, the continuous part of the true vein would augment more than that of the imaginary vein, *i. e.*, would consequently augment in greater proportion than that of the square roots of the two charges. Thus the gradual diminution in the volume of the divisions tends to render the law regulating the length of the continuous part of the vein, when the charge is made to vary, more rapid than that of Savart.

Let us pass on to what relates to the length of the divisions. As the acceleration of the velocity of the transference of the liquid forms an obstacle to the free shortening of the divisions, the latter must be gradually extended in the direction of their length, in proportion as they descend upon the continuous part. Now this gives rise to an influence exerted in the same direction as the preceding; for in consequence of their less thickness, the constricted portions will yield more readily to this traction than the dilated portions, which will necessarily increase the rapidity with which the former become diminished in thickness, and will therefore tend to produce, in each of them, the formation and rupture of the line sooner than in the corresponding imaginary vein. But the difference of the laws which the divisions and the liquid follow in their respective movements of transference, engenders an influence which acts in a contrary direction to the two preceding. In virtue of the excess which the velocity of the liquid acquires above that of the divisions, the liquid passes, as we have seen, from one division to the other, so that any one portion traverses successively, sometimes the narrower canal of a constriction, sometimes the larger space of a dilatation. But as the liquid thus moves in a conduit the dimensions of which are alternately smaller and larger, its velocity must be greater in the constricted parts, and less in the dilated parts, than if the divisions did not exist; whence this singular consequence results, that the velocity of transference of the liquid, instead of being uniformly accelerated, is subjected, in the course of the continuous part, to a series of particular variations which render it alternately greater and less than that which a solid body falling from a point situated at the elevation of the liquid in the vessel would have. Moreover, the liquid molecules, instead of moving in the direction of lines presenting a very slight curvature, and always in the same direction, as they would do if the divisions were absent, will necessarily describe sinuous lines in their passages from division to division. Now, the configuring forces emanating from the superficial layer of the vein, and which produce the divisions, cannot force

the molecules of the liquid to undergo these alternate changes of direction and velocity, without expending a part of their own action; so that things will go on as if these forces experienced a loss in intensity. If, then, the influence in question were alone exerted, the transformation would be effected with less rapidity, and therefore the continuous portion would be longer than in the corresponding imaginary vein; whence it follows, that in passing from the charge under consideration to a charge which would establish the approximative uniformity of the movement of transference of the liquid in the continuous portion, the length of this portion of the vein would increase in a less proportion than that of the square roots of the two charges.

With regard to the transference of the divisions, separately considered, we are well aware that it must be intermediate between the retarded velocity which would result from the free shortening of these divisions and the accelerated velocity of the liquid; but it would be difficult to decide, *a priori*, whether this intermediate velocity preserves any retardation or whether it presents any acceleration. However, admitting that retardation exists, the latter, tending evidently to diminish the length of the continuous portion, would produce an influence in the same direction as the above two former; and supposing, on the contrary, that acceleration occurred, this would produce an influence in the same direction as the third.

78. To sum up, then: when the charges are less considerable than those which would render the movement of transference of the liquid perfectly uniform in the continuous part of the vein, two opposite kinds of influences affect the law, according to which the length of this continuous portion varies with the charge, the first tending to make this length increase more rapidly than the square root of the charge, whilst the second, on the contrary, tends to make it increase less rapidly. Now in virtue of their opposition, these two kinds of influences will mutually neutralize each other to a greater or less extent; but in accordance with the diversity of the immediate causes which respectively produce each of these influences, complete neutralization must be regarded as very improbable; which leads us to the former conclusion, that, when the charges are sufficiently weak, the law in question will differ from that of Savart; but it will be impossible to decide *a priori* in what direction.

In the second place, the primary cause of all the influences which we have mentioned being the acceleration of the movement of the liquid, it is clear that the resulting action of those which act in the same direction, considered separately, decreases in proportion to the augmentation of the charge, and may be neglected, *commencing* with the first of the charges under which the movement of the liquid becomes perfectly uniform in the continuous portion. Now what remains of the mutual neutralization of the two resulting opposed actions is necessarily less, and probably considerably so, than each of them in particular; whence we must believe that this excess may be neglected, commencing with a much less charge. We then arrive at this second conclusion, that Savart's first law will undoubtedly begin to be true in the case of a charge which will still leave a very marked acceleration in the movement of transference of the liquid in the continuous portion.

Lastly, this result, in connexion with a principle which we have established at the end of § 73, furnishes us with a third conclusion, viz., that the charge at which the vein begins in reality to satisfy Savart's first law will be less in proportion to the size of the orifice; for it is evident that, in passing from one orifice to the other, this charge must vary in the same manner as that at which the acceleration of the movement of the liquid may be neglected. But I say further, that the variation in question will very probably take place in a much greater proportion than that of the diameters of the orifices.

For, let h' be the charge with which the approximative uniformity of the movement of transference begins in the case of a given orifice and liquid, and θ' the

corresponding value of θ. The charge h', as we have seen, should be such that $\sqrt{2gh'}$ may be very considerable in regard to $g\theta'$, or, in other words, that the proportion $\frac{\sqrt{2gh'}}{g\theta'}$ may be very great. Let us now take an orifice of less diameter, and let h'' denote the charge which fulfils in regard to this second orifice the same condition as h' with regard to the former; let, also, θ' denote what θ becomes in the case of the new orifice. If we wish, in the movement of the liquid in the continuous portion of the vein which flows from the latter, to have the same degree of uniformity as in the continuous portion of the preceding one, we must evidently make

$$\frac{\sqrt{2gh'}}{g\theta}=\frac{\sqrt{2gh''}}{g\theta''},$$

which gives

$$\frac{\sqrt{h'}}{\sqrt{h''}}=\frac{\theta'}{\theta''},$$

consequently

$$\frac{h'}{h''}=\frac{\theta'^2}{\theta''^2}.$$

But the time θ, at least in the case of mercury, is proportionate to the diameter of the contracted section, consequently to that of the orifice, (§ 74;) hence, in the case of this liquid, we may substitute for $\frac{\theta'^2}{\theta''^2}$, that of the squares of the diameters of the two orifices; whence it follows that, in passing from any determinate orifice to one which is less, the charge under consideration will decrease as the square of the diameter of the orifice. Now it must be considered as very probable that the weakest charge at which Savart's law begins to be realized will decrease in an analogous manner, *i. e.*, in a much greater proportion than that of the diameters. As we have several times stated, we are not aware whether the considerations relative to mercury are applicable or not to all other liquids; but we know at least that they are very probably so to all those the viscosity of which is very slight; consequently the above conclusion is very probably also true in regard to any of the latter liquids—such, for instance, as water.

79. Let us provisionally admit the preceding conclusions as perfectly demonstrated, and let us pass to the other law, *i. e.*, that which governs the length of the continuous portion when the diameter of the orifice is made to vary. I say, in the first place, that, in the case of mercury, this law will coincide with the second of those of Savart, when we give to the common charge the value at which the vein escaping from the largest of the orifices employed would begin in reality to satisfy the first of these laws. In fact, let us remark, first, that with the charge in question, and which we shall denote by h_1, the veins escaping from all the lesser orifices will exist *a fortiori* in the effective conditions of the first law. Consequently, if for a moment we substitute for this charge h_1 a sufficiently considerable charge to render the velocity of the liquid sensibly uniform throughout all the continuous parts, and if we again pass from this second charge to the preceding, the respective lengths of the continuous parts will all decrease in the same proportion, *i. e.*, in that of the square roots of the two charges. Now, with the largest of the latter, the lengths in question were to each other as the diameters of the corresponding orifices, (§ 74;) it will also be the same with the charge h_1; consequently with this charge the second of Savart's laws will be satisfied.

In the second place, I say that with a lower charge than h_1 the same will not hold good. To show this, let h_2 be this new charge; and let us denote by

h_3 the charge which plays the same part with regard to the vein escaping from the smallest orifice as that which h_1 plays with regard to that which escapes from the larger one. It must be borne in mind that h_3 is less than h_1, and let us suppose h_2 to be comprised between the two latter. With the charges h_1 and h_2 the vein escaping from the smallest orifice will therefore then still exist under the effective conditions of Savart's first law, whilst, as regards the vein which escapes from the larger orifice, these conditions will only commence at h_1; if, then, we pass from h_1 to h_2, the continuous portion of the first vein will decrease in proportion to the square roots of these two charges; but that of the latter vein will decrease in a different proportion. Now, with the charge h_1 these two lengths were to each other as the diameters of the corresponding orifices; with the charge h_2, then, they would exist in another proportion; consequently the second law of Savart would no longer be satisfied, at least as regards the two extreme veins of the series brought into comparison.

The following new conclusions result from all this: With a sufficiently weak common charge, the proportionality of the length of the continuous portion of the mercurial column to the diameter of the orifice does not exist throughout the entire extent assigned to the variations of this diameter; but it begins to manifest itself when that value is given to the common charge at which the vein escaping from the largest of the orifices commences to exist under the effective conditions of Savart's first law.

Respecting these conclusions, we must repeat what we stated with regard to that terminating the preceding section, viz: that they are very probably applicable, at least to all very slightly viscid liquids, consequently to water.

Now, we shall see that these same conclusions, as also those of the preceding section, are in accordance with the results of Savart's experiments, which results relate to water.

80. Savart has made two series of observations upon veins of water withdrawn from all extraneous influences, one with an orifice 6 millimetres, the other with an orifice 3 millimetres in diameter; the successive charges were the same in both series. The two following tables represent the results obtained, *i. e.*, the lengths of the continuous part corresponding to the successive charges; both the lengths and the charges are expressed in centimetres. I have inserted in each table a third column, containing, in regard to each of the lengths of the continuous part, the proportion of the latter to the square root of the corresponding charge:

Diameter of the orifice, 6 millimetres.			Diameter of the orifice, 3 millimetres.		
Charges.	Length of the continuous portion.	Proportion to the square root of the charge.	Charges.	Length of the continuous portion.	Proportion to the square root of the charge.
4.5	107	50.4	4.5	24	11.3
12	126	36.4	12	39	11.3
27	143	27.5	27	58	11.2
47	158	23.0	47	78	11.4

Before discussing these tables, we may remark here that all the lengths of the continuous portions are expressed in whole numbers; which shows that Savart has taken for each of them the nearest approximative whole number in centimetres, disregarding the fraction; hence it follows that the lengths given in these tables cannot, in general, be perfectly exact.

This being established, let us now begin by examining the table relating to the orifice of 6 millimetres. It is evident that the proportion of the length of the continuous portion to the square root of the charge diminishes considerably from the first charge to the last; whence it follows that, in the case of a vein of water escaping from an orifice 6 millimetres in diameter, if the charge be not made to exceed 47 centimetres, Savart's first law is far from being satisfied. Thus the first conclusion of § 78 is conformable with experiment. Moreover, the diminution of the proportion determines the direction in which the true law differs from that of Savart within the limit at which this begins to be sufficiently approximative; it is evident that the length of the continuous portion then augments less rapidly than the square root of the charge. In the second place, as the proportion in question increases, we find that the latter converges towards a certain limit, which must be a little less than 23, *i. e.*, the value corresponding to the charge of 47 centimetres. In fact, whilst the charge receives successive augmentations of 7.5, 15 and 20 centimetres, the proportion diminishes successively by 14, 8.9 and 4.5 units, and the latter difference is still tolerably slight in regard to the value of the latter proportion; whence we may presume that, if the charge were still further increased, the further diminution of the proportion would be very small, and that a sensibly constant limit would soon be attained, at which limit Savart's first law would be satisfied.

Let us now find the proportion of the velocity of transference of the liquid at the extremity of the continuous part to that at the contracted section, in the case of the vein escaping under a charge of 47 centimetres. We shall disregard here the small alternate variations which have been treated of in § 77, and shall therefore consider the velocity of transference of a horizontal section of the liquid of the vein as being also that which this section would have if it had fallen freely and in a state of isolation from the height of the level of the liquid in the vessel. Then, on neglecting the small interval comprised between the orifice and the contracted section, we shall have for the velocity in question, at any distance l of this section, the value $\sqrt{2g.(h+l)}$; if, then, l denotes the length of the continuous portion, the proportion of the velocity at the end of this length to that at the contracted section will be expressed generally by $\frac{\sqrt{2g(h+l)}}{\sqrt{2gh}}$, or more simply by $\sqrt{\frac{h+l}{h}}$. On now substituting in this expression for h and l the value relative to the vein in question, *i. e.*, 47 and 158, we find for the relation between the extreme velocities the value 2.1. Thus, although, under a charge of 47 centimetres, the vein escaping from an orifice of 6 millimetres may probably nearly exist under the effective conditions of Savart's first law, the velocity at the end of the continuous portion is even more than double the velocity at the contracted section, so that the movement of transference of the liquid is still more considerably accelerated. The second conclusion of § 78, therefore, appears so far to agree, like the first, with the results of experiments.

Let us pass to the table relating to the orifice of 3 millimetres. Here it is evident that the proportion of the length of the continuous portion to the square root of the charge is very nearly the same for all the charges; whence it follows that, with this orifice, the vein already begins to come within the effective conditions of Savart's first law under a charge of 4.5 centimetres. But, according to what we have stated, the orifice being 6 millimetres, the vein does not satisfy these conditions except under a charge at least equal to 47 centimetres; the charge at which Savart's first law begins to be realized, then, augments and diminishes with the diameter of the orifice, and much more rapidly than this diameter. Now, this is the substance of the conclusion of § 78.

Lastly, if, in the general expression of the relation of the extreme velocities found above, we replace h and l by the values 4.5 and 24 relative to the first vein of the table under consideration, we shall find for this relation the value 2.5; which shows that with the charge 4.5, under which the vein is already placed in the effective conditions of Savart's law, the velocity of transference of the liquid is still very notably accelerated. No doubt can, therefore, remain of the legitimacy of the second conclusion of § 78.

Let us now calculate, for each of the four charges, the proportion of the lengths of the continuous parts corresponding respectively to the two orifices; we shall thus form the following table:

Charges.	Proportions.
4.5	4.46
12	3.23
27	2.46
47	2.03

This table shows, that for charges below 47 centimetres the relation between the respective lengths of the continuous portions of two veins of water escaping, one from an orifice 6 millimetres in diameter, and the other from an orifice of half this diameter, is far from being the same as those of the diameters; whence it follows that, under these charges, Savart's second law is not satisfied. But it is evident, at the same time, that this relation converges towards that of the diameters in proportion as the charge is augmented, and that, under the charge of 47 centimetres, it nearly attains it; now, according to what we have seen above, under this same charge of 47 centimetres, the vein escaping from the larger of the two orifices very probably nearly attains the effective conditions of Savart's first law. The conclusions of the preceding section appear then to agree, as those of § 78, with the results of observation. We shall now, however, see this agreement confirmed by the results obtained with veins of water when not withdrawn from extraneous influences.

81. These extraneous influences, which consist of certain more or less regular vibratory movements transmitted to the veins, do not appear to alter the laws under consideration, considered generally; but they produce a curtailment of the continuous portions, and thus produce the same effect as a diminution of the diameters of the orifices, so that under their influence Savart's laws begin to be realized with weaker charges.

I have just stated that the complete laws which govern the continuous portion do not appear to be changed by the extraneous influences in question; this will be readily seen when, for each of the series made by Savart under the influence of these actions, in which series the orifices, the charges, and the liquid are the same as before, we construct a table of the proportions of the length of the continuous part and the square root of the charge. Notwithstanding the slight differences arising, on the one hand, from the irregularities inherent to the extraneous influences, and, on the other hand, from Savart always having given the lengths in whole numbers, we shall see that with an orifice of 6 millimetres the proportion still begins to diminish, and converge towards a certain limit; only here the limit is less, for the reason I have given above, and the limit appears to be attained under a less charge than 47 centimetres; 2d, that with an orifice of 3 millimetres the proportion is perfectly constant.

Hence the series in question may also serve for the discussion of the laws which govern the length of the continuous part. I shall limit myself here to the production of two of these series; they consist of those which Savart adopted as his type, and from which he deduced his laws. The following are the tables containing them:

Diameter of the orifice, 6 millimetres.			Diameter of the orifice, 3 millimetres.		
Charges.	Length of the continuous portion.	Proportion to the square root of the charge.	Charges.	Length of the continuous portion.	Proportion to the square root of the charge.
4.5	40	18.9	4.5	16	7.5
12	59	17.0	12	25	7.2
27	82	15.8	27	41	7.9
47	112	16.3	47	55	8.0

and the first shows that, with an orifice of 6 millimetres, the proportion of the length of the continuous portion to the square root of the charge appears to have attained its limit even with a charge of 27 centimetres; the slight increase manifested in the case of the succeeding charge is undoubtedly due to the causes of irregularity which I have mentioned.

Let us further calculate, for these two series, the proportions of the lengths corresponding respectively to the two orifices, which gives us the following table:

Charges.	Proportions.
4.5	2.50
12	2.36
27	2.00
47	2.04

It is, therefore, also under the charge of 27 centimetres that the proportion of the lengths of the continuous portions attains that of the diameters of the orifices, which completes the establishment of the conformity of the conclusions of § 79 to the results of observation.

Lastly, with an orifice of 3 millimetres, Savart has made a series of observations corresponding to four larger charges than the preceding, and the proportion of the length of the continuous portion to the square root of the charge still appeared perfectly constant; the first of these new charges was 51, and the last 459 centimetres.

82. Thus, as we have been taught by Savart's investigations, the vein gives rise to the production of a continuous sound, principally arising from the periodical shock of the isolated masses of which the discontinuous portion is composed against the body upon which they fall, and this sound may be made to acquire great intensity by receiving the discontinuous portion upon a tense membrane. On comparing the sounds thus produced by veins of water under different charges and with orifices of different diameters, Savart found that, for the same orifice, the number of vibrations made in a given time is proportionate to the square root of the charge; and that for the same charge, this number is in inverse proportion to the diameter of the orifice. We shall now see that these two laws also result from our principles.

Let us again have recourse to imaginary veins. In these the length of the divisions is equal, as we have seen, (§ 74,) to the normal length of those of a cylinder of the same liquid, formed under the conditions of our laws, and having for its diameter that of the contracted section of the vein; thus this length depends only upon the diameter of the orifice and the nature of the liquid, and does not vary with the velocity of the flow. Now it follows from this, that for the same liquid and the same orifice the number of divisions which pass in a given time to the contracted section is in proportion to this velocity, *i. e.*, to $\sqrt{2gh}$, consequently to $\sqrt{h}$. But each of these divisions furnishes lower down an isolated mass, and each of these subsequently strikes the membrane; the

number of impulses produced in a given time is equal then to that of the divisions which pass in the same time to the contracted section, and is consequently proportionate to the square root of the charge.

In the second place, as the normal length of the divisions of a cylinder, supposed to exist under the conditions of our laws and composed of a given liquid, is proportionate to the diameter of this cylinder, it follows that, for any liquid, the length of the divisions of the imaginary vein is proportionate to the diameter of the contracted section, and therefore exactly proportionate to that of the orifice. Now, for a given velocity of escape, the number of divisions which pass in a given time to the contracted section is evidently in inverse ratio to the length of these divisions; if, then, the liquid remains the same, this number is exactly in inverse ratio to the diameter of the orifice.

Thus the two laws which, according to Savart, regulate the sounds produced by the veins, would necessarily be satisfied with regard to our imaginary veins. Now, I say that the sound produced by a true vein will not differ from that which the corresponding imaginary vein would produce, if the charge is sufficient relatively to the diameter of the orifice for the velocity of transference of the liquid to augment very slightly from the contracted section to a distance equal to the length of the divisions of the imaginary vein. Then, in fact, within this extent, the two causes which tend to modify the length of the divisions, (§ 76,) *i. e.*, the acceleration of the velocity of the liquid and the resulting diminution in the diameter of the vein, will both be very small; and as they act in opposite directions, their resulting action will be insensible, so that the divisions will freely acquire at their origin the length corresponding to that of the corresponding imaginary vein. Now, it is clear that in this case the number of divisions which will pass in a given time to the contracted section will be the same in the real and the imaginary vein; consequently the sounds produced by both the veins will also be identical.

But in confining ourselves to very slightly viscid liquids, as water, we know that the relation between the normal length of the divisions of a cylinder imagined to exist under the conditions of our laws and the diameter of this cylinder must very probably differ but little from 4; consequently the same applies to the relation between the length of the divisions of an imaginary vein formed of one of these liquids and the diameter of the contracted section of this vein. If, then, in a true vein formed of one of these liquids the increase in the velocity of transference is very slight at a distance from the contracted section equal to 4 times the diameter of this section, the condition laid down above will very probably be satisfied: however, to avoid any chance of being deceived, we will take, for instance, 6 times this diameter.

It is, moreover, clear that, if the condition thus rendered precise is fulfilled with regard to a given charge and orifice, it will be so, *a fortiori*, for the same orifice and greater charges, and for the same charge and smaller orifices. We arrive, then, at the following conclusions:

1. When a series of veins, formed of a very slightly viscid liquid, flow successively from the same orifice and under different charges, if the least of them is sufficient for the velocity of transference of the liquid to augment very slightly, as far as a distance from the contracted section equal to about 6 times the diameter of this section, the number of vibrations corresponding respectively to the sounds produced by each of the veins of the series will necessarily satisfy the first of the two laws discovered by Savart.

2. When a series of veins, formed of a very slightly viscid liquid, escapes under a common charge and from orifices of different diameters, if the common charge is sufficient for the same condition to be fulfilled with regard to the vein which escapes from the larger orifice, the number of vibrations corresponding respectively to the sounds produced by each of the veins of the series will necessarily satisfy the second law. It now remains for us to show that the

above condition was satisfied in the experiments from which Savart deduced the two laws under consideration.

In the series relating to the first of these laws, the diameter of the common orifice was 3 millimetres, and the smallest charge was 51 centimetres; and in the series which refers to the second law, the value of the common charge was the same, 51 centimetres, and the diameter of the largest orifice was 6 millimetres. For our condition to be fulfilled with regard to both series, it was therefore evidently sufficient that it was so in the vein which escaped under the charge of 51 centimetres, and from the orifice the diameter of which was 6 millimetres. Now on multiplying this diameter by 0.8, we obtain for the approximative value of that of the contracted section of the vein in question 4.8 millimetres, and 6 times the latter quantity gives us 28.8 millimetres, or nearly 3 centimetres. Now if in the expression $\sqrt{\frac{h+l}{h}}$, which gives the general value of the relative proportions of the velocities of transference at a distance l from the contracted section and at this section, (§ 80,) we make $h=51$ and $l=3$, we obtain for this proportion the value 1.03; whence it is evident, that from the contracted section to a distance equal to about 6 times the diameter of this section, the velocity of transference of the liquid of the vein in question only increased 3 centimetres more than its original value.

83. Let us imagine a vein of water, and let us call a division considered immediately after its passage to the contracted section, *i. e.*, at the instant at which its upper extremity passes this section, the nascent division. It follows from what we have detailed in the preceding section, starting with a sufficient charge, that the proportion of the length of the nascent divisions of the vein in question to the diameter of the contracted section will assume a constant value, *i. e.*, independent of the charge, and that this value will very probably differ but little from 4.

Now the results obtained by Savart in the experiments relative to the laws which we have just discussed allow us, as we shall see presently, to verify the consequences of our principles.

The two opposite causes which tend to modify the length of the divisions are also those which exert an influence upon the velocity of transference, or, more precisely, upon the velocity of the transference of the necks which terminate them, (§ 76.) Now, in the case under consideration, these same causes both remaining very small throughout the extent corresponding to a nascent division, their resulting action upon the velocity of transference of the necks will be insensible throughout this extent; consequently the velocity with which a neck descends may be regarded as exactly uniform and equal to the velocity of the flow, $\sqrt{2gh}$, from the contracted section to a distance equal to the length of a nascent division.

If, then, for an orifice of a given diameter, λ denotes the length of a nascent division, and t the time occupied by a neck to traverse it, we shall have

$$\lambda = t\sqrt{2gh}.$$

Moreover, let n represent the number of divisions which pass to the contracted section in a second of time; as the time t evidently measures the duration of the passage of one of them, we shall have, taking the second as the unit of time, $t=\frac{1}{n}$, and therefore $\lambda=\frac{1}{n}\sqrt{2gh}$. Lastly, let k denote the diameter of the contracted section corresponding to the same orifice; to represent the proportion of the length of the nascent division to this diameter, we shall have the formula

$$\frac{\lambda}{k}=\frac{1}{kn}\sqrt{2gh}\,\ldots\ldots\ldots\ldots\ldots\ldots(a)$$

Now, to obtain, by means of this formula, the numerical value of the proportion $\frac{\lambda}{k}$ relative to a determined charge and orifice, we have only to ascertain, by experiment, the number of vibrations per second corresponding to this charge and this orifice; for then the value of h will be given, that of k may be deduced from the diameter of the orifice employed; we shall find that of n by taking (see preceding section) half the number of vibrations found; and, lastly, that of g is known. It is unnecessary to remark that the values of h, k, and g must be reduced to the same unit of length. Now, Savart's observations relative to the first law give us, for an orifice of 3 millimetres, the number of vibrations per second corresponding respectively to four different charges; we can calculate then, for each of these observations, the value of the proportion $\frac{\lambda}{k}$.

The following table contains these numbers, with the charges to which they refer. The latter are expressed in centimetres:

Diameter of the orifice, 3 millimetres.

Charges.	Number of vibrations.
51	600
102	853
153	1,024
459	1,843

We may conclude, from the results detailed in the note to § 74, that when the diameter of the orifice amounts to 3 millimetres, that of the contracted section is almost exactly eight-tenths of this quantity; consequently, if we retain the centimetre as the unit of length, which gives 0.3 for the value of the diameter of the orifice in question, we shall have

$$k=0.3\times 0.8=0.24.$$

Lastly, the numbers of vibrations, and therefore the values of n, supposing the second taken as the unit of time, and the values of h and k being reduced to the centimetre as the unit of length, we must make $g=980.9$.

Substituting in the formula *(a)* these values of k and g, as also those of h taken from the above table, and those of n obtained by taking the respective halves of the numbers of vibrations contained in the same table, we shall find, for the proportion $\frac{\lambda}{k}$, the four following numbers:

4.39
4.37
4.46
4.29

and we see that, in fact, these numbers closely approximate to each other, and very nearly amount to 4. The mean of these numbers, *i. e.*, 4.38, gives us then, very nearly, the constant value which, commencing with a suitable charge, the proportion of the length of the nascent divisions of a vein of water to the diameter of the contracted section of this vein assumes.

This is also evidently the value of the proportion of the length of all the divisions of the continuous portion of a vein of water to the diameter of the contracted section, when the charges are sufficiently considerable for the movement of transference of the liquid to be perfectly uniform throughout the whole extent of this continuous portion. In experimentally determining, in

the case of any other liquid, the number of vibrations corresponding to a given charge and orifice, the value of $\frac{\lambda}{k}$ referring to this liquid is also obtained by means of the formula *(a.)* If we confine ourselves to liquids, the viscosity of which is very slight, the values would very probably be found to differ but little from the preceding; and it may consequently be considered that, with the same charge and the same orifice, the sounds produced by the veins formed respectively of these various liquids are very nearly of the same pitch; but the case would undoubtedly be different, at least in general, if we passed to liquids of considerable viscosity.

Savart says that the nature of the liquid appears to exert no influence upon the number of vibrations corresponding to a given charge and orifice; but he does not point out what the liquids were which he compared in this respect; from what we have stated, it may be presumed that these liquids were some of those the viscosity of which is very slight.

84. Since the partial duration of the transformation of a cylinder may evidently be taken into account, as we have already remarked, by considering only one of the constrictions of the figure, or simply the neck of the latter, and, on the other hand, as this duration varies, for the same diameter, with the nature of the liquid, it follows that in the vein the time comprised between the instant at which the superficial section, which constitutes the neck of a constriction, passes to the contracted section, and the instant of the rupture of the line into which this constriction is converted, will also vary, all other things being equal, with the nature of the liquid. Now, it necessarily follows from this, that for the same charge and the same orifice the length of the continuous part of the vein will vary according to the nature of the liquid; and this conclusion is also in conformity with the results of experiment. In fact, as is well known, Savart has measured the continuous portion of four veins flowing under identical circumstances, and formed respectively of sulphuric ether, alcohol, water, and a solution of caustic ammonia, and he found the following lengths:

Ether	90
Alcohol	85
Water	70
Ammonia	46

85. Hitherto we have only entered upon the consideration of veins projected vertically from above downwards. Let us now consider veins projected in other than vertical directions. These are incurved by the action of gravity, and cannot, therefore, be any further compared to cylinders; but we must remark, that the phenomenon of the conversion into isolated spheres is not the result of a property belonging exclusively to the cylindrical form; it appears that this phenomenon must be produced in the case of every liquid figure, one dimension of which is considerable with regard to the two others; we have, in fact, seen the liquid ring formed in the experiment described in § 19 become converted into a series of small isolated masses, which would constitute so many spheres if their form were not slightly modified by the action of the metallic wire which traverses them. We can understand, then, that in curved veins divisions passing gradually to the state of isolated spheres ought also to be produced; consequently, the constitution of veins projected either horizontally or obliquely must be analogous to that of veins projected vertically from above downwards, which conclusion agrees, in fact, with Savart's observations.

This analogy of constitution must evidently extend to the ascending portion of the veins projected vertically from below upwards; only in the case of the latter veins the phenomena are disturbed by the liquid which is thrown back.

86. The properties of those liquid figures, one dimension of which is considerable with regard to the two others, and particularly of cylinders, furnishes then the complete explanation of the constitution of liquid veins projected from circular orifices, and accounts for all the details and all the laws of the phenomenon, at least so long as the modifications produced in it by extraneous causes, *i. e.*, by the vibratory movements transmitted to the liquid, are excluded.

As regards the mode of action of these vibratory movements, it is evident that the properties of the liquid cylinders cannot make us acquainted with them. These movements constitute a totally different cause from the configuring forces, consequently one which is foreign to the general object of our treatise; however, to avoid leaving a deficiency in the theory, we will also examine, relying upon other considerations, the manner in which the vibratory movements act upon the vein, and we shall thus arrive at the complete explanation of the modifications which result from it, and the constitution of the latter; but we shall reserve this subject for the following series.

The influence exerted by the vibratory movements communicated to the liquid led Savart to regard the constitution of the vein as being itself the result of certain vibratory movements inherent in the phenomenon of the flow. From this assumption, Savart has endeavored to explain how the kind of disturbance occasioned in the mass of the liquid by the emission of the latter might in reality give rise to vibration, and he has shown that the existence of the latter would entail the alternate formation of dilatations and constrictions in the vein. It has been shown, in the exposition of our theory, that the constitution of the vein is explained in a necessary manner by facts, quite independently of all hypothesis. We may then, I think, dispense with a detailed discussion of the ingenious ideas which we have mentioned, ideas for the complete comprehension of which we must refer to Savart's memoir itself. We shall merely remark, that it is difficult to admit the kind of disturbance supposed by Savart to occur, except during the first moments after the orifice is opened; moreover, that it is not very evident how the vibrations in question, after having traced upon the surface of the vein a nascent division, would produce the further development of the latter, so as to make it pass gradually, during its descent, to the state of an isolated mass; lastly, that to remove these difficulties, we should again be obliged to have recourse to additional hypotheses, to arrive at the laws governing the length of the continuous portion, and those to which the numbers of vibrations corresponding to the sounds produced by the shock of the disturbed portion are subject. However, it is by borrowing one of Savart's ideas, which becomes applicable when, from some external cause, vibrations are really excited in the liquid, that we find the elements requisite for entering upon the latter part of the theory.

87. In the next series, after having concluded what relates to the vein, we shall return to the liquid masses free from gravity; and we shall study the other figures of revolution besides the sphere and the cylinder, as also those figures which do not belong to this class, for which the equation of equilibrium may be interpreted in a rigorous manner.

THIRD SERIES.

Theory of the modifications which liquid veins, projected from circular orifices, undergo under the influence of vibratory movements.

§ 1. In the preceding series we deduced from the properties of our liquid figures the theoretical explanation of the constitution of liquid veins projected from circular orifices and withdrawn from all disturbing influence; it now remains to consider, also, under a theoretical point of view, the curious phenomena which are produced when vibratory movements are communicated to the liquid. Commencing our investigation, as has been already stated, with an idea announced by Savart, we shall show how these movements combine their effects with those of the configurative forces which determine the gradual transformation into isolated masses, and thenceforth all the phenomena in question will be explained in a natural manner.

After aiming to establish, by help of an ingenious theory, that the disturbance occasioned in the mass of the liquid of the vessel by the efflux itself may excite in that mass vibrations directed perpendicularly to the plane of the orifice, Savart has shown that similar vibrations would result in the formation of alternate dilatations and constrictions on the surface of the vein, because the portion of the latter which would issue during the continuance of a vibration, directed from within outwards, would undergo a compression which would increase its thickness, while the portion which issued during the continuance of a vibration directed from without inwards would undergo, on the contrary, a contraction which would attenuate it. Now, as our researches have shown, the formation of dilatations and constrictions of the vein is due to quite another cause than vibratory movements, namely, to the instability of the equilibrium of figure; but when vibratory movements are transmitted from the exterior to the liquid of the vessel, and exist, consequently, in reality in that liquid, when, for instance, we place in communication with the walls of the vessel a sonorous instrument in vibration, then the movements in question must necessarily tend to exert on the vein the action supposed by Savart; and if these movements are suitably periodical, their action will evidently concur with those of the configurative forces. We shall presently examine this more closely; but we must first return to a point of the theory which we have stated in regard to veins not submitted to that influence.

§ 2. As was seen (2d series, §§ 72, 74, and 82) when the flow takes place in the direction of the descending vertical, if we imagine the movement of translation of the liquid to be exactly uniform, the laws of the transformations of cylinders apply clearly to the vein, and we thence easily deduce the laws indicated by Savart, laws which control, we know, the length of the continuous part and the sound rendered by the impact of the discontinuous part against a stretched membrane. But this case of uniformity in the movement of translation cannot be realized; we can only approximate to it by augmenting the discharge, (*Ibid.*, §§ 72 and 73,) and, in the whole length of the continuous part, the movement of translation is always more or less accelerated; whence it necessarily results that, in the absence of configurative forces, the vein would continue to grow narrower indefinitely from above downwards. Hence, the liquid figure being no longer exactly cylindrical, the laws of the transformation of cylinders could be no longer applicable to it without some modification; and since the volume of the divisions* of a cylinder is propor-

* It will be remembered that we designate as *divisions* of a liquid cylinder the portions of that cylinder each of which is converted into an isolated sphere, and that, during the transformation, all the divisions are limited by the circles of the neck *(cercles de gorge)* of the constrictions.

tionably less as the diameter of the cylinder is smaller, it would seem that the divisions of the vein must undergo, during their descent, a gradual diminution of volume in a certain ratio with the above attenuation. Now, notwithstanding the apparent legitimacy of the inference, this was nothing more than an hypothesis, and it was improperly presented as the expression of the reality. In effect, it led, in the first place, to a consequence difficult to be admitted, namely, (*Ibid.*, §§ 76 and 77,) that the liquid descends more rapidly than the divisions, and that, moving thus in a sort of channel of dimensions alternately wider and narrower, its velocity would undergo a succession of periodical variations; moreover, if the divisions lost something of their volume in the transit of the continuous part, it would follow that the volume of each isolated mass would be less than that of an incipient division, and as the same quantity of liquid must necessarily pass, within the same time, at all distances from the orifice, the number of masses which would strike per second upon a stretched membrane would be greater than that of the divisions which would commence per second at the contracted section, a result irreconcilable, as will presently appear, with our theory of the influence of vibratory movements on the vein.

But another hypothesis may be formed equally probable, *a priori*, which does not involve the difficulties just mentioned, and which, as we shall see, is sustained by the results of experiment. Instead of regarding each division as independent of those adjacent, and as thus freely and gradually diminishing in volume by reason of the progressive slenderness of the vein, so that all those which, at a given instant, are ranged along the continuous part shall have volumes decreasing from the upper to the lower, it may be assumed with equal probability that these divisions are reciprocally dependent (*solidaires*) as regards one another, and that, in virtue of this reciprocal dependence, (*solidarité*,) they must all have an equal volume, but that, in consequence of the attenuation of the vein, this uniform volume is intermediate between those which would correspond separately to the two extreme divisions; this volume would, therefore, be proportionably less as the vein tends more to grow slender, or, in other words, proportionably less as the discharge is weaker. In this way all complication disappears; the divisions descend with the proper velocity of the liquid without modifying their initial volume; the liquid does not pass from division to division, and hence its velocity of translation does not undergo periodical variations; finally, each division which leaves the contracted section furnishes only the material of a separate mass, and consequently the number of masses which strike, in a given time, upon a stretched membrane is always equal to that of the divisions which pass, in the same time, at the contracted section. Only, when we diminish or increase the discharge, the divisions acquire, from their origin, a less volume in the former case and a greater volume in the second, a volume which they will afterwards preserve through the whole passage of the continuous part.

It is essential to remark here that these variations in the volume of the incipient divisions necessarily require corresponding variations in their length, and that hence these divisions must be shorter or longer according as the discharge is weaker or stronger.

§ 2 *bis*. We shall adopt, then, as more simple, and as harmonizing theory with facts, the new hypothesis just presented, and it will be necessary to correct in this sense § 76 of the 2d series. This hypothesis leads us, like the first, to recognize two kinds of influences acting in opposite directions on the law which determines the length of the continuous part when we cause the discharge to vary; but here, again, we shall see that matters tend to simplification.

First, let us remember that if the movement of translation were uniform, the proportionality to the square root of the discharge would still be satisfied, even beginning with very weak discharges, (2d series, §§ 72 and 75.) Now,

if the divisions descend with the accelerated velocity of the liquid, and if we suppose that there thence results no change in the duration of their transformation, they will pass, during that duration, over a more considerable space, so that the continuous part will be longer than if the acceleration did not exist, and the excess, compared with the length which the continuous part would have in the case of the uniform movement, will be considerable under a weak or moderate discharge, while it will be inconsiderable under a very strong discharge—this latter rendering the movement of translation in the continuous part sensibly uniform. Accordingly, when we pass from the first of these two discharges to the second, the ratio of the lengths of the continuous parts which respectively correspond to them will be nearer to unity than it would be if the acceleration were null; that is to say, nearer to unity than that of the square roots of the discharges.

But the divisions cannot descend with an accelerated movement without being at the same time elongated, (2d series, § 76,) and thence proceed two causes of diminution in the duration of the transformation. We have seen (2d series, § 66) that the more the length of the divisions of a cylinder surpasses the limit of stability, the greater is the rapidity of transformation; and, on the other hand, the stretching out which the divisions of the vein thus undergo must attenuate the constrictions more than the dilatations, because the former, having already a tendency to grow deeper by the action of the configurative forces, oppose no resistance to the effects of the elongation, while the latter resist by virtue of the contrary tendency. This second influence, the diminution, namely, in the duration of the transformation, a diminution which must be so much the more decided as the rapidity of translation less approximates to uniformity, or as the discharge is weaker, operates evidently to render the law more rapid than the proportionality to the square root of the discharge, and this influence is consequently opposed to the former.

Finally, there is a third influence, the inverse of the preceding, and hence of the same character with the first: as was shown at the end of the 2d paragraph, the incipient divisions must be proportionably shorter as the discharge is weaker; but, agreeably to what has been said above, this curtailment, by diminishing the excess of the length of each division beyond the limit of stability, must tend to augment the duration of the transformation. Consequently the 78th paragraph of the 2d series, relating to the neutralization of the two opposite kinds of influences, and therefore to the manifestation of the laws of Savart, remains unchanged; only it must not be forgotten that the influences to which we now refer are not altogether those which were indicated in the 77th paragraph of that series, and it will be seen that they are rather more simple. But the second part of the 82d paragraph of the 2d series, in which we sought to establish *a priori* the conditions for realizing the laws advanced by Savart in regard to the sounds which veins render, cannot be maintained, for the considerations therein set forth rest on the first hypothesis. In reasoning according to the new one we will say: for a determinate orifice, in proportion as the discharge is increased, the constitution of the vein approximates more and more to what it would be if there were no acceleration, and consequently the length of its incipient divisions verges towards that which they would acquire in the same case; whence it follows, from the first part of the same paragraph 82, that starting with a less discharge sufficiently strong, the laws of Savart will be necessarily satisfied. This is all that the new hypothesis can furnish us on the subject of the conditions in question; it does not enable us to determine the least discharge under which these conditions begin to be fulfilled; for it does not give the precise elements for calculating the length of the incipient divisions.

Lastly, the commencement of § 83 of the 2d series, which establishes, upon the other hypothesis, the approximate uniformity of the movement of transla-

tion of the necks (*cercles de gorge*) of the constrictions in the small extent which corresponds to an incipient division, must be also rectified. On the new hypothesis, the movement of translation of the *cercles de gorge* is the movement itself of the liquid, and we can calculate consequently with exactness, for the discharge and orifice employed by Savart, how much the velocity has increased at a distance from the contracted section six times the diameter of that section, that is to say, evidently greater than the length of an incipient division, and the increase thus obtained scarcely exceeds a hundredth. The new hypothesis therefore establishes, as well as the first, the nearly exact uniformity of the movement of translation of the *cercles de gorge* in the small extent in question, and consequently all the rest of the paragraph is justified.

§ 3. These rectifications having been made, we proceed directly to the subject. We will first briefly recapitulate what, according to the researches of Savart, are the modifications which the vein experiences under the circumstances we are considering, that is, when it is under the influence of vibratory movements. The first fourteen of the following Nos. relate to veins descending vertically.

1. The continuous part is shortened.

2. The thickness of the limpid portion seems augmented.

3. Each of the masses which are isolated at the lower extremity of the continuous part is first flattened in the vertical direction, and consequently its horizontal diameter is greater than that of the sphere which it tends to constitute,

4. The masses being thus abandoned to themselves under a flattened form, and tending to assume the spherical form, they afterwards exceed this latter through the effect of inertia, and are lengthened in the vertical direction, are then flattened anew, and again elongated, and so on in succession; so that their horizontal diameter, which was at first greater than that of a sphere of the same volume, becomes afterwards less than this latter, then again greater, &c. These periodical variations of the horizontal diameter of the masses taking place while the latter are borne forward by their movement of translation, the impression left on the eye by the rapid passage of any one of these masses must be that of a figure presenting a regularly arranged series of maxima and minima of thickness, the former corresponding to the places by which the mass has passed at the moments of its greatest horizontal development, and the latter to the places by which it has passed at the moments of greatest horizontal contraction; and since the successive masses pass, either exactly or nearly so, by the same places in the same phases of their oscillations of form, the impression which they individually produce are more or less completely identified, and the discontinuous part of the vein presents in a permanent manner the differences of thickness in question; in other words, this discontinuous part appears to be composed of a regular series of elongated expansions and nodes occupying fixed positions. When the above superposition is imperfect, each expansion presents the appearance of an assemblage of films, of which each constitutes a species of cone having for its axis that of the vein. About the half of the first expansion is formed by the passage of the dilatations of the base of the continuous part, so that this continuous part terminates about the middle of the length of that expansion.

5. The length and the diameter of the expansions are so much the more considerable as the discharge is stronger and the diameter of the orifice greater, and the same is the case as regards the diameter of the nodes.

6. This assemblage of phenomena is manifested even when the vein is left to itself under the ordinary circumstances, that is, when vibratory movements are not designedly excited in the liquid of the vessel. This results, on the one hand, from the circumstance that the impact of the discontinuous part upon the liquid in which it falls occasions vibrations which are transmitted to the vessel

through the intervention of the air and the supports, and, on the other hand, from the vessel also receiving through the supports the slight vibrations due to external sounds communicated by the ground. It is only by withdrawing the vessel, by means of certain expedients, from these two influences that the vein assumes the aspect proper to it.

7. But all the phenomena enumerated under the first five of the preceding numbers become much more decided and regular when, by help of an instrument, we produce, in the vicinity of the apparatus, a sound in consonance with that which would result from the shock of the discontinuous part of the vein against a stretched membrane. The continuous part is then considerably shortened; the diameter of the limpid portion is increased, the dilatations, being still further massed upon themselves, grow larger, so that the nodes which separate them are more elongated, and appear of smaller diameter.

8. Besides the above unison, other sounds, produced by an instrument in the neighborhood of the apparatus, act upon the vein in an analogous manner, but with much less energy. There are also sounds which exert no influence.

9. In the particular case where the instrument varies very little from unison, the continuous part of the vein is lengthened and shortened alternately, and the ear is sensible of beats which coincide with those variations of length.

10. When the discontinuous part of the vein is received on a body which can render only a determinate sound, it frequently happens that the vibrations of that body modify the sound proper to the vein; but this appears impossible unless the interval between the latter sound and that which agrees with the body impinged upon does not exceed a minor third. When the sound of the vein is thus modified by a foreign sound, it frequently requires only, in order to cause a return to the tone which pertains to it, a slight blow on the apparatus or a change of position in the body impinged upon, and it is always by abrupt starts that the return is effected. If the interval between the two sounds be very slight, they may make themselves heard periodically or even simultaneously.

11. The modifications which the vein experiences under the influence of the vibratory movements still increase and acquire a perfect regularity when the sonorous instrument, (7,) instead of being at a certain distance from the apparatus, is placed in contact with the walls of the vessel and renders a very intense sound exactly in unison with that which is proper to the vein. The continuous part is then so much abridged that the upper extremity of the first dilatation almost touches the orifice, and, further, the superposition of the expansions formed by the individual masses (4) is exact, so that no appearance of films is longer perceptible.

12. This extreme regularity enables us clearly to distinguish the apparent figure produced by the passage of the spherules interposed between the masses, a figure which occupies the axis of the vein from the extremity of the continuous part; here also may be observed expansions and nodes, but shorter than those which are due to the passage of the masses.

13. By means of an instrument thus placed in contact with the walls of the vessel, almost all sounds can produce effects analogous to those of unison with the tone proper to the vein; but these effects are less decided in proportion as the sound of the instrument varies more from the unison in question.

14. Further, under this condition, when the sound which is natural to the vein is not in accord with that of the instrument, it may be brought to be so, even when the variance between the numbers of vibrations is sufficiently great to constitute an interval of a fifth above or more than an octave below the sound proper to the vein.

15. If the vein, instead of flowing vertically from above downwards, is projected horizontally, and is left to the ordinary circumstances, or, in other words, is not under the influence of a sonorous instrument, but is allowed to strike the liquid of the vessel which receives it, the discontinuous part presents expan-

sions and nodes such as, in the same circumstances, are presented by that of vertically descending veins, (6,) and the vibrations of an instrument modify it in the same manner. If the vein be ejected obliquely from below upwards, the same phenomena are still observed, so long as the angle which it forms with the horizon does not exceed 20° to 25°.

16. But beyond that limit, and as far as 45° to 50°, the discontinuous part assumes other aspects: when the vein is not under the influence of the sound of an instrument, this discontinuous part appears spread out on one vertical plane into a sort of sheaf. Under the action of vibrations of a definite period, it may happen that the sheaf is resolved into two quite distinct jets, having each its expansions and nodes regularly formed; it may even be that, for another definite sound, the sheaf shall be replaced by three jets; finally, there is always a sound which reduces the entire vein to a single jet, presenting a system of expansions and nodes perfectly regular, and this sound is also that which produces the greatest shortening of the continuous part.

17. For the same discharge and the same orifice, the number of vibrations corresponding to the sound which exerts the maximum of effect on the length of the continuous part and on the dimensions of the expansions of the vein, is so much less as the direction in which this last is ejected makes a greater angle with the descending vertical drawn from the orifice. The difference between the numbers of vibrations which correspond with the case in which the jet falls vertically and with that in which it is projected horizontally, is inconsiderable; but it becomes very great between this last case and that in which the jet is an ascending vertical.

§ 4. Let us proceed now to the explanation of these singular phenomena. What follows, as far as § 24, will relate to veins ejected in the descending vertical, and, up to that point, such veins must constantly be kept in view.

Experiment has taught us (2d series, § 46) that, in the transformation of a liquid cylinder the length of a constriction is exactly, or at least almost exactly, equal to that of a dilatation, and we shall hereafter demonstrate that this equality is strictly exact at the commencement of the phenomenon; now this result is evidently applicable to the incipient constrictions and dilatations of the vein, and it follows that the respective durations of the passage of one of these constrictions and of one of these dilatations at the contracted section are equal; on the other hand, a division of a cylinder or of a vein being comprised between the centres of two neighboring constrictions, and hence being composed of a dilatation and two semi-constrictions, the duration of the passage of a division of the vein at the contracted section is necessarily equivalent to the sum of the durations required for the passage of a dilatation and a constriction; and since these two last are equal, we arrive at this first consequence, that the duration of the passage, whether of a constriction or a dilatation, at the contracted section, is equal to half that of the passage of a division.

But the number of vibrations per second corresponding to the sound rendered by the impact of the discontinuous part of the vein upon a stretched membrane is, we have seen, (2d series, § 82,) double that of the isolated masses which, in the same interval of time, strike upon that membrane, and, in virtue of our new hypothesis, (§ 2,) this last number is always equal to that of the divisions which pass, within the same time, at the contracted section; hence the duration of each of the vibrations in question is, like the duration of the passage of a constriction or a dilatation, equal to half that of the passage of a division, and we thence deduce, finally, this fundamental conclusion: *The duration of each of the vibrations corresponding to the sound proper to the vein is equal to that of the passage of a constriction or of a dilatation at the contracted section.*

§ [illegible]. Let us suppose now that, through the means indicated by Savart, we have withdrawn the vein from the influence of the vibrations caused by the fall of the liquid into the vessel which receives it, and from that of external causes; and that then, the vein being left to the sole action of the configurative forces, we transmit to the vessel from which it escapes, and consequently to the liquid contained therein, a sound exactly in unison with that which would be rendered by the impact of the discontinuous part against a membrane. The liquid which flows from the interior of the vase towards the orifice, passes through it under the action of the resulting vibrations; and if these are communicated in a vertical direction, each portion of the vein which escapes at the contracted section, under the influence of a descending vibration, will be propelled by the velocity $\sqrt{2gh}$ increased by that of the vibration, and will, consequently, contain more liquid than the portion which would have passed in the same time in the absence of the vibrations. The excess of velocity will tend, it is true, to be communicated to the part of the vein situated below that which we are considering; but, putting out of view for a moment the configurative forces, we must at least admit that this inferior part will oppose a certain resistance in virtue of its inertia, and that, therefore, the excess of liquid, superinduced by the excess of velocity, will tend to disperse itself in a horizontal direction, or, in other words, to dilate the portion to which it pertains.

This being premised, if the nearly cylindrical figure which the vein would assume under the sole effects of the movement of translation of the liquid and of the circular form of the orifice was a stable figure of equilibrium, the portion which, under the action of the descending vibration, dilates while it passes at the contracted section, would at the same time exert an effort to return to its first form; whence it necessarily follows, upon the hypothesis in question, that in proportion as the dilatation is formed, it would be propagated to the subjacent sections, and would constitute on the surface of the vein a dilated wave of a certain length, which would advance with a velocity equal to the sum of the velocity of its own propagation and of that of the liquid. Then also the portion of the vein which would afterwards pass at the contracted section under the action of an ascending vibration, and which would consequently traverse that section with the velocity $\sqrt{2gh}$ diminished by that of the vibration, would produce, for the opposite reason, a constricted wave of the same length with the dilated wave, and which would advance behind the latter with the same velocity; there would then come a new dilated wave followed by a new constricted one, and so on, as long as the communication of vibratory movements was continued. But, by reason of the instability of the cylindrical figure and of the tendency of the vein to transformation into isolated spheres, things will pass in quite another manner. Let us imagine that the lower extremity of one of the dilatations, which would be formed by the sole action of the configurative forces due to the instability, should traverse the contracted section at the precise moment when a descending vibration commences in the liquid. Now, since the configurative forces impel in a continuous manner into this portion of the vein an excess of liquid which dilates it, without any tendency in the vein to contract upon itself, we see that the quantity of liquid superinduced at the same time by the additional velocity due to the descending vibration may be distributed in the horizontal direction and contribute to the formation of the dilatation, without having to surmount a contrary tendency. Moreover, since the duration of the vibration is equal to the time which the portion of the vein, which the configurative forces would themselves alone form an incipient dilatation, employs in passing at the contracted section, the upper extremity of that portion will traverse the contracted section at the precise moment when the vibration terminates, so that the immediate action of this vibration will be exerted on the whole portion in question, and only on that portion. In fine, since the dilatation produced by the combined actions of which we have just spoken has

no tendency to become effaced, it will not be propagated to the subjacent parts, and consequently will not give rise to a wave. Hence the portion of the vein under consideration will be more dilated from the first than it would have been in the absence of vibratory movements; but it will have the same length and descend with the same velocity as in this latter case.

After the descending vibration will come an ascending one, and this latter diminishing the velocity of the passage at the contracted section, there will result, as has been already remarked, in the portion of the vein which passes under its influence, a diminution of volume, so that this portion will tend to become narrower; but the configurative forces tending to make of this same portion an incipient constriction, the attenuation due to the vibration will be effected without encountering an opposite tendency, and consequently without giving rise to the formation of a wave. Thus we see that, as in the case of the dilatation which precedes it, the constriction formed by the double action of the configurative forces and of the vibration will be less decided, but will have the same length and descend with the same velocity as if the vein were abandoned to the sole action of the configurative forces.

In fine, the same thing will take place in regard to all the other dilatations and constrictions: in virtue of the equality between the time occupied by each of these portions of the vein in passing at the contracted section and the duration of each vibration, all the dilatations will coincide with the descending vibrations, and all the constrictions with the ascending vibrations; all will consequently preserve their length and their velocity of translation, but all will quit the contracted section more distinctly defined, or, to use other words, in a more advanced phase of transformation, than if vibratory movements had not been produced.

§ 6. But the action of these movements will not be limited to this: in effect, the velocities of the ascending and descending vibrations—velocities which, as we have shown, change direction in the dilatations and constrictions to produce a greater transverse development of the former and a greater attenuation of the latter—cannot be annihilated, in each of these portions, at the moment when its passage at the contracted section is finished; these velocities thus changed into transverse velocities will continue therefore, as acquired velocities, to form an addition to those which result from the configurative forces.

§ 7. In order that the transmitted vibrations shall exert with full intensity on the incipient divisions of the vein the action described in the two preceding paragraphs, it is necessary that at the orifice they should have, as we have supposed, a vertical direction. It would be difficult, doubtless, to show *a priori* that in being propagated to the orifice the vibrations really take that direction; but Savart, who has been so much occupied with the communication of vibratory movements, admits the fact implicitly: he supposes, in effect, that on the one hand these vibrations only reinforce those which arise, according to him, from the efflux itself and which would necessarily be vertical, and on the other he does not say that, to obtain the maximum of action, it is necessary to give to the sonorous instrument any particular position. For the rest, if we find therein some difficulty, it would suffice to remark that whatever be the real direction in which the liquid molecules, in traversing the orifice, execute the vibrations transmitted to them, we may always, save in the wholly exceptional case in which that direction is exactly horizontal, decompose each vibration into two others, of which the horizontal one will exert no influence on the transformation of the divisions of the vein, while the other and vertical one will exert its whole action.

We have supposed, moreover, that the moment when each descending vibration commences is also that when the lower extremity of each dilatation passes at the contracted section; but if, at the first instant when the vibrations make themselves felt, this coincidence does not take place, there will be a conflict

between the action of the configurative forces and that of the vibrations, and it may be readily conceived that then the transformation of the vein, which being only a phenomenon of instability may be displaced by slight causes, will cause the collective dilatations and restrictions to retrograde or advance, so as to establish very soon the above coincidence and thus allow the concurrence and entire freedom of the two systems of action.

§ 8. These principles being established, we shall see emerge from them one by one all the modifications undergone by the vein through the influence of the vibrations.

Let us first remember that when the vein is abandoned to the sole action of the configurative forces the velocity with which the transformation is accomplished remains very small to a quite considerable distance from the contracted section, which gives to the corresponding portion of the vein a calm and limpid aspect; in the second place, that, further on, the dilatations acquiring a marked and more rapid development, the vein appears to grow larger up to the point at which the masses become isolated; and, finally, that beyond that point the diameter of the vein, a diameter which is that of these masses, is sensibly uniform. (2d series, § 70.)

Let us suppose now such a vein, and produce, in proximity with the apparatus, the sound we have been considering in all that precedes. Under the influence of this sound each division quitting the contracted section in a more advanced phase of transformation, (§ 5.) and the transformation moreover advancing from that phase with a greater velocity than it would have done under the sole influence of the configurative forces, (§ 6,) it necessarily results that this transformation will be completed in less time; consequently each division will attain the state of an isolated mass at a less distance from the orifice, and thus the continuous part will be rendered shorter. And since the dilatations are more developed from their inception, we see, in the second place, that the apparent thickness of the limpid portion of the vein, a thickness which, at each point in the length of that limpid portion, is evidently that which the dilatations have acquired at the moment of passing it, will be augmented. In the third place, the excess of transverse velocity which the transformation receives from the vibrations, and which continues as acquired velocity, must necessarily cause the horizontal diameter of the successive masses to exceed that of the spheres which these masses tend to constitute, so that the masses will become flattened in the vertical direction. But we perceive that this horizontal extension and vertical flattening render the capillary pressure, at the circumference of the mass, superior to that of the points near the axis, and that thence arises an increasing resistance which ends by destroying the transverse velocity. Then the differences of pressure will act freely, and the mass will return upon itself to attain its figure of equilibrium, that is, the spherical figure; but the phenomenon, being effected with an accelerated velocity, cannot stop at this last figure, and the mass will be contracted in the horizontal direction while it is elongated in the vertical direction, until the increasing resistance which results from new inequalities between the pressures has annihilated the acquired velocity; the mass being now urged by the differences of pressure which have produced that resistance will again return towards the spherical figure, which it will once more surpass, and extending itself a second time in the horizontal direction will become flattened in the vertical one; after which it will recommence the same series of modifications, and will continue these oscillations of form as long as its fall continues.

Thus we explain very simply, for the case of unison with the sound which the impact of the discontinuous part would occasion, the facts recited in Nos. 1, 2, 3 and 4, of § 3. Only, as the extremity of the continuous part of the vein occurs about the mid-length of the first expansion, and consequently is little distant from the point corresponding to the first of the maxima of thickness of

the discontinuous part, we perceive that each mass must attain its first phase of greatest horizontal development a little before it completely detaches itself, and at the moment, no doubt, when it clings to that which follows it only by a thread. As to the system of films of which the expansions present the appearance when the phenomena are not altogether regular, this, as Savart has stated, is evidently the result of the inexact superposition of several expansions individually produced by successive masses: these expansions are then seen simultaneously, and are observed as though through one another, from the effect of the persistence of their impressions on the retina.

§ 9. It is clear that the time comprised between two phases of the strongest horizontal contraction, or, in other words, that which each mass occupies in accomplishing a complete oscillation of form, is independent of the velocity of translation; consequently, the interval which a mass traverses during the time in question is proportionably greater as the velocity of translation is more considerable; but this interval is evidently the distance which separates the centres of two nodes, or the length of an expansion;* this length must therefore increase with the discharge.

The volume of the incipient divisions thus increasing with the discharge, and each of these divisions furnishing an isolated mass, the volume of these masses must equally increase with the discharge; now the more volume these masses have, the greater must be their horizontal diameter in its successive maxima and minima; but these maxima and minima diameters are respectively the diameters of the expansions and nodes; hence the diameters of the expansions and those of the nodes must also augment with the discharge. Only this augmentation tends towards a limit of but little extent; for the greatest volume which the isolated masses can acquire is evidently that which they would assume were the movement of translation of the liquid uniform, that is to say, were it that of the spheres into which might be resolved an indefinite cylinder formed of the same liquid and having a diameter equal to that of the contracted section. (2d series, § 74.)

Now, if the quantity does not vary, but we use a larger orifice, the volume of the divisions of the vein, and consequently that of the isolated masses, will be more considerable; but the greater these masses, the less rapid must be their oscillations of form, and therefore the more space must they traverse in their descent, during one of these oscillations; hence the length of the expansions must increase with the diameter of the orifice. As to the respective diameters of the expansions and nodes, it is evident, from what has been remarked above, that they will increase at the same time. We see, therefore, from the contents of this paragraph, that the facts of No. 5 of § 8 are necessary consequences of the theory; always, however, upon the supposition that the vibrations are of the same period with those of the sound proper to the vein. We pass now to the facts of Nos. 6 and 7.

§ 10. When the vein is not under the influence of a sonorous instrument, but is received in a vessel placed upon the ground, the principal cause of the vibratory movements transmitted by the air and the supports to the vessel from which the discharge takes place is the impact of the isolated masses against the liquid into which they fall; we perceive then that, in these movements, vibrations must prevail of the same period with those which would result from the impact of the masses in question against a stretched membrane, and consequently the action exerted upon the vein is explained by what has been stated in the preceding paragraphs. But the vibrations thus produced not having a great intensity, the modifications of the vein cannot acquire all

* It is thus that Savart seems to consider the expansions when occupied with their length, and we conform to his expressions in the following paragraph; but, in reality, it is obvious that the space in question is composed of an expansion and two half nodes.

the development of which they are susceptible; besides, these vibrations being but little regular, and being accompanied by slight and still more irregular vibrations which proceed from external noises, the phenomena cannot fail to be impressed by these irregularities, and it is under these circumstances, in effect, that Savart describes the appearance of films in the interior of the expansions.

Savart has approximately measured, under the circumstances in question, in veins of water ejected by two different orifices and with different discharges, the lengths and diameters of the expansions as well as the diameters of the nodes. We deem it not superfluous to reproduce here the results of those measurements, in which the centimetre is assumed as unity:

Orifice of six millimetres diameter.

Discharges.	Lengths of the continuous part.	Lengths of the expansions.	Diameters of the expansions.	Diameters of the nodes.
4.5	40	25	0.9	0.70
12	59	30	1.0	0.75
27	82	39	1.1	0.80
47	112	60	1.2	0.90

Orifice of three millimetres diameter.

Discharges.	Lengths of the continuous part.	Lengths of the expansions.	Diameters of the expansions.	Diameters of the nodes.
4.5	16	7.8	0.50	0.28
12	25	9	0.52	0.32
27	41	13	0.55	0.36
47	55	16	0.60	0.40

We will here remark that the length of an expansion being the space traversed by a mass during the continuance of one oscillation of form, and that continuance being constant in the same vein, the expansions pertaining to this latter must, because of the acceleration of the descent, increase in length, beginning with the first. It is strange, therefore, that Savart, who, in another part of his memoir, speaks of this increase in reference to a particular experiment, should have given, in the above tables, the lengths in question as absolute; we must presume that they relate to the first expansion of each vein. In fact, the experiment in which Savart observed the increase of length of the expansions would render the effect more apparent, because the first expansion occurred very near the orifice.

§ 11. If, while the vein falls freely in the liquid of the vessel which receives it, we cause an instrument which yields a unison, as has been supposed in the preceding instances, to sound in proximity with the apparatus, then, under the action of these more intense and perfectly regular vibrations, the modifications of the vein will be necessarily more distinct; that is, the limpid portion will appear a little thicker, the continuous part will undergo a new shortening, the expansions will be enlarged and the nodes reduced. Moreover, the expansions individually formed by each of the masses will be superposed in a more exact manner, and hence will less overreach one another towards their extremities, so that the expansions which result from them collectively will be more

massed on each other, and the nodes which separate the latter will seem to be elongated. And such, in reality, as we see by No. 7 of § 3, is the state of the vein under the influence in question.

The phenomena would be much more regular still if the vein were originally withdrawn from all extraneous influence, and Savart, in fact, speaks of the great regularity of the expansions which show themselves when such a vein is received on a stretched membrane, which then serves as a sonorous instrument yielding an unison.

§ 12. When the instrument, which is made to resound in the vicinity of the apparatus, yields a tone not in unison with that appropriate to the vein, the vibrations no longer succeeding one another at the same intervals occupied by the passage of the dilatations and constrictions due to the configurative forces, there can be no uninterrupted concurrence between the two species of action; but neither can these be incessantly in conflict, and it is obvious that from this alternation of accord and opposition must result effects of a very complicated kind. We will attempt, however, to discriminate to a certain point what then occurs in the vein.

To simplify as far as possible, we will suppose extraneous action to be previously annulled. During the succession of the phenomena let us seize, in thought, the instant when the middle of a constriction due to the configurative forces traverses the contracted section precisely in the middle of the duration of an ascending vibration; this vibration will then evidently concur with the configurative forces to deepen the constriction. But if the sound of the instrument is sharper than that of the vein, and the vibration has therefore less duration than the passage of the constriction, a part, more or less considerable, of the base of the latter will have been in conflict with the end of the descending vibration which has preceded, and an equivalent part of the summit will be equally in conflict with the commencement of the descending vibration which follows, since these descending vibrations tend to dilate the portions of the vein on which they act. If, on the contrary, the sound of the instrument is graver than that of the vein, it is clear that the concurrence will take place for the whole of the constriction, but that the commencement of the vibration will have been in conflict with the upper part of the preceding dilatation, and that the close of this vibration will be in conflict with the lower part of the succeeding dilatation.

It is easy to perceive that after a certain number of vibrations an identical effect will be produced; that is, that the middle of an ascending vibration will coincide anew with the middle of the passage of a constriction, that the same thing will still recur after a number of vibrations equal to the preceding, and so on, periodically, at equal intervals. If, for instance, the duration of a vibration be $\frac{5}{6}$ of that of the passage of a constriction or a dilatation, the total duration of six double vibrations, each composed of an ascending and a descending vibration, will be equivalent to the total duration of the passage of five constrictions and five dilatations; now, it is easy to perceive that if we commence computing this duration at the instant of one of the above coincidences, it will also terminate at the instant of a like coincidence; in our example, therefore, the coincidences will be reproduced successively after intervals equal to the duration of six double vibrations. Let us now endeavor to discover what passes during each of these intervals, or, in other words, between one coincidence and the following.

With that view let us examine what takes place at the moment when the first half of one of these intervals terminates. In the example we have taken we shall evidently be then at the middle of an ascending vibration; but if we reflect that the interval commences at the passage of the inception of a division (§ 4) and exactly comprises the passage of five entire divisions, we shall recognize that the end of its first half is the instant of the passage of the middle

of a division, and, consequently, of the middle of a dilatation; hence, for this entire vibration there will be opposition with the configurative forces; this will be the maximum of the conflict, and it is obvious that this conflict will, till then, have gone on augmenting; that is to say, occupying greater and greater portions of the successive vibrations, to diminish afterwards by the same degrees. These principles being granted, let us see what can be deduced from them.

Each of the constrictions for which a coincidence shall exist will quit the contracted section in a more advanced phase of transformation, and hence will be broken at a less distance from the orifice than if vibratory movements had not been produced; but the following constriction, which is under less advanced conditions, can only be broken a little further on, and the subsequent ruptures will, in like manner, be effected further and further from the orifice, until the rupture of the constriction in regard to which the conflict between the two actions is at its maximum; after which things will proceed in an inverse order; that is, the successive places of rupture will reascend till there recurs anew a constriction with a coincidence, when all will recommence in the same order as at first. It appears then that, in such a vein, the continuous part has different lengths, which succeed one another periodically; but the shortest of these lengths must be regarded as being that of the real continuous part of the vein, since the continuity is there persistent, and it is necessarily smaller than would be the continuous part of the same vein not subjected to the influence of a sonorous instrument. Still the shortening will not be so great as in the case of unison. In effect, if the sound of the instrument is sharper, the most complete concurrence between the two kinds of action only takes place, as has been said above, with the middle portion of the constrictions upon which it falls, and there is conflict in the extreme portions. If the sound of the instrument is graver, the concurrence extends, indeed, to the whole of the constriction, but the conflict then exists in the adjacent portions of the two dilatations between which this constriction is comprised, and these portions admitting with less facility the liquid which is driven towards them, the constriction cannot obey with entire freedom the two actions which tend to narrow it.

In the second place, from what has been just said, the shortening should be proportionably less as the sound of the instrument deviates more from unison; for the more it is above this, the less is the portion of the constriction for which a concurrence exists; and the more it is below, the further does the conflict extend over the two neighboring dilatations. And since in the constrictions for which coincidence exists, and to a certain distance on both sides of each of them, the action of the vibrations favors, more or less, that of the configurative forces, the vein will present, in an analogous but less decided manner, the other modifications which unison determines; hence the limpid portion will still appear a little thickened, and the interrupted part will have expansions and nodes; but these modifications will be the less decided as the interval between the sound of the instrument and consonance is greater.

We may, therefore, so far as the complexity of the subject permits it, pronounce substantially the four following conclusions: When, at a certain distance from the apparatus, a sound is produced sharper or graver than that which is proper to the vein, first, the continuous part will assume periodically different lengths; second, the shortest of these lengths, which is that of the true continuous part, will be less than was the length of the sole continuous part before the action of the instrument, but this curtailment will not be so great as in the case of unison; third, the vein will present, in a manner analogous to that which takes place under the action of unison, but at the same time less decidedly, a small increase of thickness in the limpid portion and a system of expansions and nodes in the interrupted part; fourth, all these phe-

nomena will be the less decided as the sound of the instrument deviates more from unison, so that the sounds which depart too much from that unison, whether above or below, will appear inoperative.

We have supposed that extraneous action has been previously neutralized; but this action tending of itself to occasion like effects, (§ 10,) it will be understood that if we allow it to subsist, it can scarcely but add to the intensity of the phenomena. We may here take notice that sounds, differing from unison, give rise at the same time to effects of another kind, effects which will, in general, be little apparent in veins directed vertically, but which are manifested, as will be seen, in those whose emission takes place under certain obliquities. These effects depend on the conflict between the vibrations and the configurative forces, and are consequently null in the case of unison; they cannot, therefore, like those we have been considering, go on decreasing from that point, but, on the contrary, it is with the departure from unison that they are developed.

§ 13. The first of the four conclusions above stated is clearly verified, in a particular case, by the fact of No. 9 of § 3. In effect, when the sound of the instrument is very nearly consonant, the duration of a vibration differs very little from that of a dilatation or a constriction, and consequently when a coincidence shall occur, it will be almost complete; that is to say, the conflict will occupy only extremely small portions of either the constriction or the two adjacent dilatations; for such a constriction, therefore, the effect will be nearly the same as if there were exact unison, whence it follows that at the moment of the rupture of this constriction the continuous part of the vein will have perceptibly the length which corresponds with unison; it will then acquire, progressively, greater lengths until that is reached which corresponds to the maximum of conflict; but on account of the approximate equality of the respective durations of a vibration and of the transit of a dilatation or a constriction, it will evidently be only after a sensible space of time that this maximum will present itself, so that the gradual elongation of the continuous part will be effected with sufficient slowness to be followed by the eye; and such will necessarily be the case with the next and the succeeding curtailments of length. As to the beats, it is plain that they result from the mutual reaction of the sound of the instrument and of that of the vein; for although Savart does not say so in express terms, we may conclude from the manner in which he states the fact in question, that the vein must fall on a stretched membrane.

Except in this particular case of a very small interval between the sound of the instrument and that of the vein, Savart says nothing of periodical changes of the length of the continuous part, and not without reason, as we shall see. For intervals which do not meet the above condition, these changes are too rapid to allow their succession to be distinguished, insomuch that all the lengths must seem simultaneous as well as all the systems of expansions corresponding to those lengths; each of the expansions of the vein, therefore, must, in these circumstances, appear to be formed of individual expansions not exactly superposed, and consequently present the aspect of an assemblage of films, (§ 8;) now, there was nothing new in this aspect for Savart, who had observed it (§ 10) in the expansions of veins not subjected to the influence of a sonorous instrument.

§ 14. The three other conclusions of § 12 seem confirmed by No. 8 of § 3. Yet the manner in which Savart mentions the facts might cast some doubt on the entire exactness of that agreement; we shall therefore give *verbatim* the only passages which relate to the facts in question:

"Sounds which form the grave octave and fifth, the minor third, the superfluous fourth, and the shrill octave of that rendered by the impact of the interrupted part against an auxiliary body, produce in the vein modifications analogous to those just described, [those, namely, produced by unison,] but with much less energy; and there are sounds which do not act in any manner on its dimensions and the aspect it presents."

And afterwards, in speaking of a vein received at a very small distance from the orifice on a thick solid body:

"In this case, as well as when the vein is entire, we observe that the grave and high octaves, the fifth and the sharp minor third of the sound in question—that is, of unison—may influence, though in a less degree, the state of the vein."

And again, in reference to the modifications experienced, under the influence of the consonance due to the impact against a stretched membrane, by a vein withdrawn from every other extraneous influence:

"Analogous results are obtained when, with a stringed instrument, different sounds are produced in the vicinity of the reservoir, but one of the sounds always exerts upon the vein a greater influence than all the others."

Do these passages signify that besides unison it is only the grave octave and fifth, the minor third, the superfluous fourth, and the shrill octave which modify the state of the vein? That is little probable, for then, instead of saying, "there are sounds which do not act in any manner," &c., Savart would have said, *all the other sounds which precede them are without influence, &c.* Must we interpret these passages as admitting that the sounds therein specified are the most active after unison, and that among the remaining tones of the gamut, some are simply less efficacious, while others absolutely exert no influence? But in that case, can we believe that Savart would have thus expressed himself? We remark, moreover, that the superfluous fourth mentioned in the first passage is omitted in the second.

These vague statements show that Savart had but little studied the influence of other sounds than unison, at least under the circumstances which we are considering, and it appears to us that there could no more be deduced therefrom the existence of any disagreement between our theoretical conclusions and the facts, than that of an absolute accordance. Fortunately Savart contrived afterwards to augment the energy of action of the vibrations produced by the instrument, and then the effects, such as he describes them, must be regarded as wholly conformable with our conclusions, as will presently be seen.

§ 15. To finish what regards the influence of a sound excited at a distance and different from unison, we have still to account for the facts of No. 10 of § 3. We shall proceed to show that these facts, excepting the last, depend on a more general principle, which may be stated in the following manner: if the vibrations of the instrument are sufficiently energetic in relation to those occasioned by the impact of the isolated masses, and if at the same time the interval of the two sounds is not too great, the sound of the vein may be brought to unison with that of the instrument. We observe that these circumstances are those of the number cited: in effect, when the vein falls on a body which can only render a determinate sound, such as a diapason, if we suppose for an instant that it undergoes no modification in the number of the isolated masses, the vibrations due to the impact of these masses will be generally of another period than those of the body struck, and consequently they can only proceed from the circumstance that each time a mass reaches that body the air is driven from between them, then returns, to be expelled anew on the arrival of the following mass, and so on in succession; but the sonorous waves produced in this manner are necessarily very weak relatively to those produced by the vibrations of the body struck; besides, we have it in our power, by varying either the discharge or the diameter of the orifice, to diminish as much as we please the interval of the two sounds.

The vibrations of the instrument, (or, in the case before us, of the body impinged upon,) transmitted by the air to the vessel and the liquid, not having the same duration with the transits of the incipient constrictions and dilatations due to the configurative forces, there is, as has been shown, (§ 12,) a variable conflict between these two kinds of action; but, if the two sounds do not

differ too much from one another, we can conceive that the transformation of the vein, a phenomenon susceptible of being influenced by extraneous causes, (2d series, § 58,) may, under the action of the vibrations, lengthen or shorten the incipient constrictions and dilatations, in such manner that the duration of the passage of each of them shall be precisely equal to that of a vibration, and that the two kinds of action shall be constantly in accord; this point being attained, the sound of the vein will be necessarily in unison with that of the instrument. Only, for the vibrations of the instrument to be capable of realizing that result, it is obviously necessary that they should have a sufficient degree of energy relatively to the vibrations of the sound proper to the vein, since these last tend to favor the normal action of the configurative forces.

We shall better comprehend the phenomenon by considering it under a point of view a little different. Let us remember that the vibrations tend of themselves to produce, in the vein, incipient constrictions and dilatations, (§ 5;) now, if these constrictions and dilatations are a little superior or a little inferior in length to those which the configurative forces on their side tend to originate, and if moreover the action of the vibrations is energetic enough to control that of these forces, the system of incipient constrictions and dilatations which will be formed must be that which depends on the vibrations, and hence the transformation thus modified at its origin will be completed after this new manner.

But this state of the vein is a forced one, since the natural mode of transformation is altered. Hence, if something suddenly disturbs the succession or regular transmission of the vibrations, the configurative forces will at once become again preponderant, and the incipient contractions and dilatations will resume the length which corresponds with the free action of those forces. Thus is explained without difficulty the statement of No. 10, § 3, that it often requires but a slight blow given to the apparatus or a change in the position of the body struck suddenly to restore the sound of the vein to the tone which is proper to it. We have supposed that, in the experiment referred to, the sound of the vein is restored to unison with that of the body receiving the impact, conformably with the principle advanced at the beginning of this paragraph. Savart, however, as may be inferred from the statement in the number in question, does not express himself in this respect in precise terms: he merely says that the sound of the body struck modifies that of the vein, that it changes its period; but other experiments which we shall presently discuss authorize us to ascribe to these words the sense we have given them.

§ 16. We further learn from No. 10 of § 3, that when the difference of the two sounds is very small, these two sounds may make themselves heard periodically or even simultaneously. Let us try likewise to explain these facts.

We will suppose, for the sake of distinctness, that the sound proper to the vein is somewhat graver than that of the body struck. In the case of exact unison, the number of impulses of the masses in a given time would be half the number of the vibrations of the body in the same time, and consequently the interval between two successive impulses would be equal to the duration of two of these vibrations; therefore, upon the above supposition, the interval between two impulses will a little exceed the duration of two vibrations, and if the reaction of these vibrations on the incipient constrictions and dilatations is not sufficiently powerful to modify the length and thus produce unison, the small excess of duration of the intervals in question will be maintained. This premised, let us begin with the first impulse. This will cause the body to perform a vibration directed from above downwards, which will be followed by a vibration from below upwards; then, a little after the commencement of a new descending vibration, the second impulse will arrive; the third will act during the third descending vibration, but at a little more advanced phase of that vibration; the fourth impulse will take place during the fourth descending vibra-

tion and at a phase still a little more advanced: and so on, until an impulse precisely coincides with the termination of such a vibration. Under these repeated impulses, the amplitude of the vibrations of the body will necessarily go on increasing, as far as the impulse last cited. But, by virtue of the small excess of duration of the intervals, the impulses which follow will occur during the ascending vibrations, and, as before, at phases more and more advanced, so that after a number of impulses equal to that of the preceding series, the body will again be struck at the instant of the termination of a vibration. Now, this second group of impulses will evidently destroy the effects of the first; that is to say, will gradually diminish the amplitude of the vibrations and end by annulling them. A third group of impulses will revive these vibrations, a fourth will annul them anew, and so on indefinitely. The sound of the body struck will be therefore alternately raised and lowered; on the other hand, the sound of the vein will be weaker when the masses reach the body during its descending vibrations than when they strike it during its ascending vibrations, on account of the difference of the relative velocities: and we see, moreover, that this latter sound has its minima during the augmentations of that of the body, and its maxima during the diminutions. This being so, if the vibrations of the body acquire, in their greatest amplitude, a certain energy, and if the relative velocity of the impulses becomes at the same time sufficiently small, the sound of the vein will be entirely masked at the moments of greatest intensity of that of the body, to reappear and predominate in turn at the intermediate moments; and consequently the two sounds will be heard periodically. But if the body is capable of executing vibrations of only small amplitude, and if it be held at a great distance from the orifice, it may be that the relative velocity of the impulses shall continue to be always considerable, so that the sound of the vein will be perceptibly uniform, and that of the body, in its maxima, not have sufficient intensity to mask it. In that case, the first will not cease to be perceived, and consequently, during the periods of intensification of the second, they will be both heard at once. It is doubtless in this sense that we should interpret the words, *or even simultaneously*, which are literally borrowed from Savart.

§ 17. Let us now revert to the case where a sonorous instrument is made to render a sound in exact unison with that proper to the vein. If the instrument, instead of acting at a distance, is placed in contact with the walls of the vessel whence the vein escapes, it is clear that the vibrations communicated to those walls and propagated in the liquid will be much more energetic, and that, in consequence, the modifications of the vein will be much more decided; moreover, the small irregularities spoken of in § 10 will be then entirely effaced. The contents of No. 11 of § 3 are thus explained of themselves.

§ 18. Proceeding to No. 12 of § 3, we observe, in the axis of the vein, on quitting the lower extremity of the continuous part, another system of expansions and nodes more minute as well as shorter, which is due, as Savart has shown, to the spherules which accompany the masses.

Here an apparent difficulty presents itself. When the vein is withdrawn from all vibratory action, its interrupted part is free from expansions and nodes; it would seem, therefore, that under the sole action of the configurative forces, the masses arrive at the spherical form without perceptible oscillations, and that the oscillations of form take place only in the case in which the configurative forces are re-enforced by vibrations; now, the mode of production of the spherules can in no manner be influenced by the vibrations, for these act directly only at the contracted section; lower down than that section, their effect is limited to the acquired velocities, (§§ 6 and 8,) which accelerate the development of the dilatations and the deepening of the constrictions, then to the conversion of each of these last into a thread, and this thread is afterwards transformed, thus furnishing the spherules by the configurative forces alone, which arise therein as in every liquid cylinder sufficiently elongated; nevertheless,

these spherules pass through oscillations of form, since the trace of their passage before the eye presents expansions and nodes.

For the purpose of elucidating this point let us examine, attentively, what are the circumstances in regard to the spherules and in regard to the large masses. Let us remember (2d series, § 62) that the thread generally separates into three parts, of which the two extreme ones reunite themselves respectively with the two large masses between which the thread is comprised, while the intermediate one contracts itself at once and symmetrically from above and below, dilating at the same time horizontally so as to produce the spherule in question. By virtue of this simultaneousness and symmetry of action, the small portion of liquid attains the spherical form towards which it tends, but it does so with an acquired velocity and thus necessarily overpasses it, so that its vertical diameter becomes less and its horizontal diameter greater than the diameter of the sphere of the same volume; hence the oscillations of form of the spherules, and consequently the expansions and nodes which result from them.

Things do not occur, however, after identically the same manner with the large mass suspended to the thread and which is isolated by its rupture; in effect, a moment before this separation the mass in question was already rendered free at its lower part by the rupture of the thread formed between it and the mass which precedes it; here, then, the ruptures below and above the mass, and, of course, the two contractions which tend to flatten it in the vertical direction, do not take place at the same time; besides, as each of these contractions must be followed by an elongation, neither do these take place simultaneously, and the same is the case consequently with the contractions and elongations which follow. Thus each contraction from the bottom of the mass will be effected wholly or in part while an elongation is taking place above, and *vice versa;* but the first tends to increase the horizontal diameter of the mass and the second to diminish it; their effects on this diameter will, therefore, more or less destroy one another, and if there be no vibratory influence which, by the accession of velocity which it imparts to the transformation, shall carry the diameter in question beyond that of the sphere and thus determine an excess of pressure to the equator of the mass, this diameter will vary but little, and consequently we shall observe no system of expansions and nodes in the discontinuous part of the vein. We see that, even under the sole action of the configurative forces, the masses which become isolated at the extremity of the continuous part are necessarily the seat of oscillations of form, though these oscillations can only exist in a marked degree in the vertical direction. We have, therefore, committed a slight error in § 69 of the 2d series, by saying that, after being isolated, the masses at once form themselves into spheres.

§ 19. Let us return, for an instant, to the spherules. When a thread is transformed, the small constrictions therein produced become themselves changed into still more slender threads, each of which breaks at two points, and thus furnishes, by its middle portion, an exceedingly small spherule, (2d series, § 62.) These last spherules are frequently thrown beyond the axis of the vein, impelled, no doubt, by the movements of the air; but as their mode of generation is the same with that of the less minute spherules of which we took notice above, they also must undergo oscillations of form, and Savart assures us that this is the case, though without indicating by what means he verified it: the parabolic trajectory described by such of these spherules as are projected beyond the vein leaves probably on the eye a trace sufficient to allow the observation therein of expansions and nodes; it may be also possible, perhaps, to distinguish the apparent figure resulting from the passage of those which maintain their position in the axis.

§ 20. Let us now produce anew a sound which differs from that of the vein, still continuing to place the sonorous instrument in contact with the vessel, so as to give more energy to the action of the vibrations. We perceive, by No. 12 of § 3, that in this case the last three conclusions of § 12 are distinctly in accordance with the observations of Savart. There may, it is true, seem something vague in the words, *almost all sounds;* but they cannot be supposed to signify that ineffectual sounds alternate with effectual ones. Let us suppose, in effect, for an instant, the inefficacy of certain intermediate sounds, and imagine that the tone of the instrument goes on deviating in a continuous manner from that of the vein; then, when we quit one of these inefficacious sounds, it will be necessary either that the action on the vein, from being null as it was for this sound, increases gradually to a certain point, which would be contrary to the statement of the number cited, according to which the action diminishes in proportion as we depart from unison; or else that this action becomes suddenly energetic, which is scarcely admissible. It is very probable, therefore, that the idea of ineffectual sounds, implied in the words, *almost all sounds*, refers simply to sounds too far remote from that of the vein, which, in virtue of the statement in question, must produce but an insensible action.

§ 21. It was said, § 15, that vibrations differing in period, within certain limits, from those of the sound proper to the vein, may predominate over the configurative forces in the generation of the incipient constrictions and dilatations; that the transformation thus commenced is then completed after this new manner, and that, consequently, the sound of the vein is reduced to unison with that of the instrument. Now, the most favorable condition for the production of this result must evidently be the contact of the sonorous instrument with the walls of the vessel, because of the more immediate transmission of the vibrations. And, in effect, while in the case of No. 10 of § 3, the phenomenon can only be realized in an interval of a minor third, here, as we see by No. 14 of the same paragraph, it extends to intervals of a fifth above its principal sound and of more than an octave below; we may add that Savart does not employ here, as in the former case, terms of little precision; he says distinctly that the sound of the vein is reduced to unison with that of the instrument.

§ 22. An upper limit, so high as the fifth, seems, at first glance, to be in opposition with certain results of our second series. In effect, for the sound of the instrument to ascend a fifth, it is necessary that the number of isolated masses which strike, in a given time, against the stretched membrane, should increase in the ratio of 2 to 3, and that, consequently, (§ 2,) it should be so likewise with the number of incipient divisions which pass, in the same time, at the contracted section, and as, under a constant discharge, the length of the incipient divisions is evidently in inverse ratio with this latter number, it follows that, from the principal sound to its fifth, the incipient divisions become shortened in the ratio of 3 to 2; but we know (2d series, § 83) that when a vein of water renders the sound proper to it, the length of its incipient divisions is equal to 4.38 times the diameter of the contracted section;* if, then, by the sole action of a sonorous instrument, the sound of such a vein rises by a fifth, the length of its incipient divisions will be reduced to $\frac{2}{3}$ of the above value; that is, to 2.92 times the diameter of the contracted section; now, this number is a little inferior to the limit of stability of liquid cylinders, a limit which, as has been shown, (2d series, § 46,) is comprised between 3 and 3.6,

* Such, at least, is the value of the ratio under moderate or strong discharges; under a weak one, the incipient divisions taking, in virtue of the hypothesis of § 2, a less volume, and consequently a less length, the ratio would be also less. But we are led to the conclusion that, in the experiments in question, the discharge employed by Savart was not of this latter kind.

and yet it was demonstrated (2d series, § 57) that when a liquid cylinder is transformed, the length of its divisions cannot be less than that limit.

The difficulty is but apparent. The demonstration cited supposes that the transformation of the cylinder commences spontaneously, and then it is strictly true; but it does not apply to the case in which the constrictions and dilatations are originally formed by an extraneous cause sufficiently energetic. In effect, the demonstration in question consists essentially in showing that if, in the first phases of the transformation, we consider the sum of a constriction and a dilatation—a sum whose length is equivalent to that of a division—all passes in that portion of the cylinder as if its two bases were solid, so that the transformation cannot be established spontaneously without a separation of those bases at least equal to the limit of stability; but if, in a cylinder realized between two solid disks whose distance is a little less than the limit of stability, the transformation could not commence of itself, it is clear that it will continue of itself if it has commenced from an extraneous cause which has accumulated the liquid in a certain quantity towards one of the disks, so as to occasion artificially a dilatation and a constriction sufficiently decided, for evidently, at the limit of stability, and in passing from beyond to within it, there is no sudden transition from instability to an absolute stability. When that limit is passed, the stability must at first be very feeble, since it parts from zero; consequently, at but little distance within the limit, a deformation impressed artificially on the cylinder can only be effaced spontaneously if it be small; if the deformation be considerable, it will proceed, on the contrary, spontaneously, and will produce the disunion of the mass. The demonstration which we have recalled can, therefore, be no longer cited when, in a liquid vein, the incipient constrictions and dilatations are formed by energetic vibrations. Then, if the sum of the lengths of one of these constrictions and one of these dilatations, or its equal, the length of a division, is a little inferior to the limit of stability, the transformation can commence after that anomalous mode, (?) and the more intense the vibrations, the more will the last sound for which the possibility of the phenomenon exists be elevated above the principal sound. If the extraneous sound is below the principal sound, and thus tends to give to the incipient divisions a length necessarily superior to the limit of stability, it will not encounter the kind of resistance which has just been indicated within that limit, so that the possibility of the phenomenon will extend much further; we see, in effect, that in the experiments of Savart it embraces an interval of more than an octave.

There is still another reason why the phenomenon should be less limited below the principal sound than above: in the same sonorous instrument, the amplitude of the vibrations increases generally with the gravity of the sound; but the more considerable the amplitude of the vibrations transmitted, the greater is the excess of liquid which each descending vibration tends to drive into the vein to form an incipient dilatation, and the greater also is the withdrawal of liquid which each ascending vibration tends to effect and thus deepen an incipient constriction. If, then, in proportion as the sound of the instrument departs from the principal sound, whether below or above, the length of the incipient divisions which the vibrations tend to form becomes more and more superior or more and more inferior to that of the incipient divisions which the configurative forces tend on their part to form, and if thence there evidently arises a conflict of progressive intensity with these latter forces, on the other hand, below the principal sound, the vibrations act more and more energetically to cause the new mode of transformation to prevail, and this augmentation of action must more or less countervail the augmentation of the conflict.

We may remark here, that in the case of a sound very grave relatively to the principal sound, the new mode of transformation is not established after the same manner as in the case of a sound which does not much deviate from

the principal sound; in the latter case, in effect, because of the little difference of length between the incipient divisions of the two kinds, it is quite probable that the configurative forces simply modify their proper action, as has been already said in § 15, by elongating or shortening the incipient divisions which correspond with them, so as to make them coincide with those which correspond with the vibrations; but when the sound of the instrument is sufficiently grave for the length of these last divisions to surpass considerably that of the others, when, for example, the instrument renders the grave octave, and the vibrations transmitted are sufficiently intense to impress upon the vein their mode of transformation, it must be admitted that the action of the configurative forces is completely destroyed, so that there is no longer a modification of the first mode, which adapts itself to the second, but an absolute substitution of the second for the first.

§ 23. Experiment fully verifies what has been said above of the variations of stability within and near the limit, in a liquid cylinder adhering to solid bases. A horizontal cylinder of oil was formed, in the interior of the alcoholic mixture, between two disks* whose diameter was 31 millimetres and their distance 87 millimetres; the ratio of length to the diameter was therefore, in this cylinder, equal to 2.8, so that the figure was quite stable; this ratio, we see, deviated somewhat more from the limit than that which we found, in the preceding paragraph, to pertain to the incipient divisions of a vein of water brought by the action of a sonorous instrument to render the sharp fifth of the principal sound. In order to alter artificially the cylindrical form of the mass, the point of the small syringe was moved slowly, and at several intervals, along the upper part of the liquid figure, starting from one of the disks and stopping at very nearly the middle of their interval; the oil thus accumulates in greater quantity towards the other disk, and, during this whole operation, the figure ceases not to regulate itself spontaneously in relation to its axis; that is to say, it remains one of revolution, so as to present a constriction and a dilatation analogous to those which result from a spontaneous alteration. Now, so long as the versed sine of the meridian arc of the dilatation was less than about 5 millimetres, the mass, if left to itself, gradually recovered the cylindrical form; but when the sine in question attained 5 millimetres, the mass left free continued spontaneously to change its form and ended by disuniting.

In this experiment, the artificial deformation necessary to determine the spontaneous continuation of the phenomenon is considerable; for when, by approximate measurement, the versed sine of the meridian arc of the dilatation was 5 millimetres, that of the meridian arc of the constriction was 8 millimetres, so that the respective diameters of the neck of the constriction and of the equator of the dilatation were 15 and 41 millimetres, and hence the first was scarcely more than the third of the second; but let it be remembered that the ratio between the length and the diameter of the cylinder was below that which, in the vein, corresponds to the fifth of the principal sound.† Moreover, there are two other reasons why the passage of the sound of the vein to the sharp fifth should be induced by vibrations which occasion directly a deformation much less decided. In the first place, from the immediate action of the vibrations, the deformation must increase by the acquired velocities, (§ 6;) and in the second place, the divisions, and consequently the constrictions and dilatations, being elongated during their descent, (§ 2 *bis*,) the sum of the lengths of a constriction and a dilatation, inferior at first to the limit of stability, begins imme-

* These disks were maintained by a system similar to that represented in *Fig.* 27 of the second series.

† This ratio, but for an error in the construction of the small apparatus, would have been 2.92.

diately to approach that limit, so that the progress of the transformation after the anomalous mode originally impressed becomes more facile.

§ 24. Thus the theory accounts for all the phenomena resulting from the action of vibrations on veins ejected in a descending vertical, for all those at least which Savart describes in a precise manner. We pass to veins ejected in other directions. And first, since, in these veins, there is equally a transformation into isolated masses, sounds must necessarily exert on them an influence analogous to that which they exert on veins ejected vertically from above downwards; No. 15 of § 3 has therefore no need of explanation.

§ 25. But this is not the case with No. 16. If all the divisions, on attaining one after the other the extremity of the continuous part, became isolated in identically the same manner, and if all the masses parted from thence with the velocity precisely corresponding to the movement of translation of the liquid at that point, these latter would all describe exactly the same trajectory, and then the discontinuous part of the vein could present no dispersion or sheaf-like jet; there are irregularities, then, as Savart remarks, in the emission of the isolated masses of the extremity of the continuous part; yet these irregularities must be very small, as the sheaf has no great extent. I had thought at first that they proceeded from the same causes with those which were considered in § 10. But if that were so, the suppression of the extraneous action would cause the sheaf to disappear and thus reduce the whole vein to a single jet; but this is what experiment has not confirmed: by employing, in regard to such a vein, the means used by Savart in the case of descending vertical veins—that is to say, by receiving the discontinuous part on a thick board, suitably inclined, and by placing soft bodies under the vessel from which the vein issues, under that in which it is received, and under the supports, I have not succeeded in producing any considerable diminution of the sheaf. We must infer from this that the irregularities are not owing to the vibratory movements, and that, consequently, they affect the action itself of the configurative forces. We perceive, in effect, that, considering the nature of the phenomenon of transformation, even slight disturbing causes must have an influence on the perfect identity of all the divisions which arise one after the other at the contracted section; we have seen, for example, in the experiments of §§ 50 to 55 of the 2d series, an extraneous cause alters the equality of length of the divisions of a cylinder. This premised, we proceed to show that small differences of this nature in the incipient divisions of a vein, ejected under a suitable obliquity, must necessarily give rise to a certain dispersion of the discontinuous part.

Let us consider particularly two of the constrictions with the dilatation which they comprise between them. As we have seen, each of these two constrictions, at first very feebly indicated on quitting the contracted section, afterwards deepens gradually in the transit of the continuous part, by transferring half of its liquid to the dilatation; this then receives, by its anterior extremity, a portion of the liquid which is driven in a direction contrary to the movement of translation, and, by its posterior extremity, a portion which is driven in the same direction with that movement, so that its velocity of translation tends to be diminished by the first and increased by the second of these accessions. Now, although these two opposite actions are in general unequal, because the anterior constriction is, at each instant, in a little more advanced phase of transformation than the posterior, yet if the two constrictions were perfectly identical at their respective inceptions, and if, in the sequel, they have undergone identically, though not precisely at the same instant, the same modifications until their respective ruptures, it is evident that after these two ruptures, that is to say, at the moment when the dilatation exists in the state of an isolated mass, the sum of the quantities of movement supplied to this mass by the anterior constriction will have been absolutely compensated by that of the

quantities of movement which have been supplied, in the other direction, by the posterior constriction, and that hence this mass will quit the continuous part with the velocity exactly corresponding to the general movement of translation. But it is clear that the compensation will be no longer entire if the two constrictions differed in their inception; if, for example, they were unequal in length: it results from the less duration of the transformation when the divisions are longer, (2d series, § 66,) and when, consequently, the constrictions are longer, that the more elongated of the constrictions in question will deepen more rapidly than the other, and as, in virtue of its excess of length, it comprises more liquid, it will convey into the dilatation a greater afflux of material with greater velocities, and consequently a greater quantity of movement. If, then, this constriction is the posterior one, the mass will quit the contracted section with an excess of velocity, and if the anterior, with a defect of velocity. Thus, slight differences of length in the incipient constrictions will result in establishing small inequalities in the velocities of the successive isolated masses; but these masses will then, necessarily, traverse parabolas of unequal amplitude, and will, consequently, be spread out in a vertical plane, thus forming the sheaf.

This explanation supposes that the disturbing causes do not produce, in the constrictions, any irregularity in directions perpendicular to the axis of the vein; and we are led, in effect, to conclude, from the experiment of § 23, that the constrictions and dilatations tend with great force to a symmetry in relation to the axis, and that hence irregularities in a direction perpendicular to this latter cannot be persistent. It is clear, also, from this explanation that there are two extreme limits for which the dispersion is necessarily null, namely, when the vein is ejected vertically from above downwards and vertically from below upwards, since, in these two cases, all the isolated masses perform the same rectilinear trajectory;* if, therefore, we pass from the first to the second by gradually varying the direction in which the jet is thrown, the sheaf cannot begin to show itself in a very distinct manner except on attaining a certain angle between that direction and the descending vertical, and it will cease to be very distinguishable beyond a certain other angle. Moreover, so long as the vein is thrown in directions descending obliquely, and even in a horizontal direction, it will be readily conceived that at the extremity of its continuous part, a part which is generally of quite considerable length, it will already approach too nearly to the vertical to allow a very clearly marked sheaf, so that the first direction which will begin to render the sheaf distinct will be one ascending obliquely. All these conclusions are in accordance with the facts of the number we are considering.

We admit, it will be seen, that the inequalities between the incipient constrictions do not depend on the direction in which the jet is thrown; and there is no plausible reason, in effect, for attributing these inequalities to the ascending obliquity of the jet. If we have not spoken of them in treating of veins descending vertically, it is because, in the latter veins, they cannot give rise to any appearance of a peculiar kind; they then do no more than evidently a little augment, in the axis of the vein, the inexactness of the superposition of the individual systems of expansions and nodes, and thus simply constitute an influence to be added to those mentioned in § 10. As to the nature of the disturbing causes which produce the inequalities in question, it would doubtless be difficult to discover it; but, whatever it be, the dispersion of the discontinuous part in veins directed under a suitable angle reveals to us the presence of those causes.

* In a vein ejected vertically from below upwards, the liquid scatters, it is true, in falling back; but I need not remark that this latter dispersion is owing to a wholly different cause, and has nothing in common with the phenomena we are considering.

§ 26. Now, a vein being projected under such an angle that the sheaf shall be well formed, let us submit it to the influence of a sonorous instrument. The sound which will most shorten the continuous part will still be evidently that whose vibrations succeed one another at the same intervals which the constrictions and dilatations due to the configurative forces (§§ 5 and 12) observe in their passage at the contracted section. But these vibrations being perfectly regular and isochronous, they will prevent, if they have sufficient intensity, the disturbing causes from modifying the incipient constrictions; in other terms, in influencing the transformation, they will impart to it their own regularity, so that all the incipient constrictions will have the same length, and hence all the isolated masses will follow identically the same trajectory (§ preceding;) under the influence of this sound the sheaf will disappear, and the whole of the vein be reduced to a single jet presenting a very regular system of expansions and nodes.

§ 27. As to the singular effects of reduction of the sheaf to two or three jets under the influence of other sounds, it would be necessary, in order to attempt an explanation, to know the relations of the sounds in question with the principal ones—relations which Savart nowhere indicates. But as these phenomena are not the least curious of those which result from the action of vibrations on liquid veins, I have decided to attempt this investigation.

The orifice I employed had a diameter of 3 millimetres; it was pierced in the centre of a circular plate of brass of 12 centimetres diameter,* so inclined that the jet might be projected at an angle of about 35° above the horizontal; this plate formed one of the bases of a cylindrical drum which communicated by a horizontal tube, wide and short, with the lower part of one of Mariotte's large vases; the discharge was of 34 centimetres, the sonorous instrument was a violoncello, the base of which was made to rest on the supports of the apparatus.

The sheaf being well developed, the attempt was made in the first place to ascertain by approaches the principal sound, or, in other words, that which precisely reduced the whole vein to a single jet with a regular system of expansions and nodes, and which, at the same time, caused the first expansion to arise very near the orifice. This point being attained, the sound of the instrument was raised by successive semi-tones. Under the influence of the vibrations thus communicated, the jet first lost its regularity, next the sheaf gradually reappeared, and afterwards was maintained without being reduced to either two or three jets. A return was then made to the principal sound, and from that point the sound of the instrument was caused to descend, likewise by semitones. The same effects, the alteration, namely, of the regularity of the jet and the progressive reappearance of the sheaf, were now manifested; but, on approaching the grave octave, a tendency to the change of the sheaf into a double jet was remarked, and when this last sound was reached, the sheaf was distinctly replaced by two jets with regular systems of expansions and nodes. The sound continued to be lowered, and the two jets still to appear, until the third below the grave octave was attained; still lower, and so long as the double grave octave was not reached, sometimes two and sometimes three jets were obtained; the fifth, however, sometimes yielded a single jet; finally, for the double grave octave, three jets were constantly observed. In all these cases, the jets continued each to have its system of expansions and nodes.

These facts are less restricted than those stated in No. 16 of § 3; according to that number, in which the purport of Savart's expressions is reproduced, it

* A diameter so considerable was employed from the necessity of leaving sufficient liberty to the vibrations of the plate. Without that liberty, the vibrations of the liquid which flows towards the orifice would be impeded, and would hence lose some of their action on the vein.

would be only under the influence of the principal sound that the sheaf would be contracted into a single jet, and there would be only two other definite and different sounds which would cause to appear respectively two and three distinct jets. But the absence of an indication of the relation between these sounds and the principal one suffices to show that Savart has not given close attention to the phenomena of this kind, and that after having observed them in isolated cases, he did not inquire whether they were susceptible of extension.

§ 28. Let us see now if the theory will account for these phenomena. We will begin with the grave octave. For this sound, the duration of a vibration is double that of the passage of a constriction or a dilatation at the contracted section, whence we may conclude without hesitation that the divisions which would originate under the sole action of the grave octave of the principal sound would be double the length of those which would be produced by the isolated action of the configurative forces. From this we may admit that each of the former comprises exactly the sum of two of the latter; for in this way, at all the sections which terminate these sums or couples there is evidently an absolute concurrence between the two kinds of action, the sections in question constituting at once the centres of the constrictions which would result from the vibrations, and centres of the constrictions due to the configurative forces. Now, let us examine what will pass, during the transformation, in any one of these couples of divisions. The couple being composed of two entire divisions, contains two dilatations which comprise between them a constriction, and is terminated by two semi-constrictions. Now, while the entire constrictions to which these terminations pertain are, as we have seen, favored by the vibrations, it is plain that the intermediate constriction is, on the contrary, in conflict, since its middle, which is the middle of the couple, corresponds to the middle of the division which the vibrations tend to produce, and consequently to the middle of the dilatation of the latter; thus each of the dilatations which the configurative forces give rise to in the vein is adjacent to two dilatations unequally solicited. Moreover, the constrictions favored by the vibrations must be elongated under their influence, since the constrictions which the latter would of themselves produce would have a length twice greater, and as the length of each of the couples of divisions above considered remains the same as in the absence of the sound of the instrument, it follows that the constrictions intermediate to the preceding, that is, those which occupy the middle of the couples and which are in conflict with the vibrations, must be shortened. We may therefore admit that the favored constrictions, although, from the beginning, they are more slender than the constriction in conflict, still contain, because of their excess of length, more liquid than the latter; and since, for the double reason that they are longer and are accelerated by the vibrations, they arrive more rapidly at their rupture, we perceive that they will transmit to the dilatations more matter with more velocity, and consequently a greater quantity of movement. All the dilatations will thus be found in the condition analyzed in § 25, and consequently the isolated masses, on abandoning the continuous part, will have some a small excess of velocity and the others a small deficit of velocity. But here the vibrations, imparting their regularity to the phenomena, render all the favored constrictions, at their origin, identical among themselves, and in like manner render identical among themselves all the constrictions in conflict, so that all the masses formed by the dilatations which, in the line of the continuous part, have behind them a favored constriction, leave with the same excess of velocity and consequently describe the same trajectory, and all the masses which proceed from dilatations having a favored constriction before them, leave with the deficit of velocity and also describe the same trajectory; hence, under the influence of the grave octave of the principal sound, the sheaf ought to be replaced by two separate jets.

Yet it would not be impossible that the sound under consideration might cause the sheaf to disappear; in effect, this sound being already very grave, at least in regard to the vein upon which I operated, its vibrations have much amplitude, and might (§ 22) act with sufficient energy to prevent the formation of the constrictions in conflict, and thus leave in the vein only the divisions which they tend of themselves to produce, in which case all the isolated masses would necessarily have the same velocity, namely, the normal velocity.

Let us examine, in the second place, the influence of the grave fifth of the preceding sound, or, in other words, of the double grave fifth of the principal sound. The vibrations of this double fifth being three times less rapid than those of the principal sound, it may readily be concluded that each of the divisions which they tend of themselves to occasion in the vein comprises exactly three of the divisions due to the configurative forces. We see, moreover, that, of the three dilatations contained in this assemblage of divisions, the last has behind it a favored constriction and before a constriction in conflict, while the foremost has, on the contrary, before it a favored constriction and behind a constriction in conflict, and, finally, that the intermediate one is between these two constrictions in conflict, which are identical with one another at their respective origins. Hence the quantities of movement will necessarily be distributed, in the isolated masses proceeding from these three divisions, in such manner that the last will quit the continuous part with a velocity superior to the normal velocity, the foremost will acquire a velocity inferior to this normal velocity, and the intermediate will quit with the normal velocity itself; and as, still on account of the perfect regularity of the vibrations, the circumstances are identically the same in each of the systems of three divisions, there can be, in the discontinuous part, but three different velocities. If, then, the action of the vibrations do not mask entirely that which, before its influence, the configurative forces freely exerted, the sheaf will be resolved into three distinct jets; and if, on the contrary, the action of the configurative forces is completely controlled, which ought to take place even more easily than for the grave octave, on account of the still greater amplitude of the vibrations, there will be but one jet, as we have shown to be the case above.

As to the separation into two jets, under the influence also of the double grave fifth, a result which experiment equally yields, we can account for it in the following manner: When the action of the vibrations is preponderant, and there arise, at the contracted section, only the divisions which it determines, these have a greater length, since each of them occupies the place of three of the divisions which the configurative forces would form; but we know (2d series, § 35) that every liquid figure, of which one dimension is considerable relatively to the two others, tends to separation into isolated masses; we can conceive, then, that in the divisions in question, if the acquired transverse velocities are not sufficient to oppose it, there may be new configurative forces developed which separate each of these divisions into two others by hollowing out a constriction in its middle, and, as all constrictions thus produced are evidently in conflict, the reasoning employed in regard to the grave octave shows that we ought then to obtain two jets.

Let us remark here, that the anomalous configurative forces, to which we have just had reference, could form, in each large division, only one constriction; if they formed two, which would separate each large division into three small ones, these would have the same length with the divisions of the vein not submitted to the influence of the sonorous instrument; but, for this to be possible, it would be necessary that the new divisions should not experience more resistance in their formation than in the absence of all extraneous action; for we may conclude from what takes place in cylinders (2d series, §§ 58 and 59) that, in every liquid figure, more or less analogous, the length of the divi-

sions increases with the resistance; now, the acquired transverse velocities causing, in our large divisions, a tendency to persevere in the mode of transformation impressed by the vibrations, constitute a resistance to any further separation.

We pass, in the third place, to the double grave octave. Here, each of the divisions which would arise under the sole action of the vibrations, would evidently comprise four of the divisions which would result from the configurative forces alone. Now, if these two actions were combined, it would seem that we should have four distinct jets; for it is easy to see that in the three constrictions which would then be formed, the conflict would be unequal; that it would be stronger for the middle constriction than for the two others, so that each of the two dilatations comprised between these three constrictions would receive from the two sides unequal quantities of movement, and that the differences, in fine, would be greater for the two extreme dilatations, each of which would be comprised between a constriction in conflict and a favored constriction. But, on the one hand, the vibrations in question having a considerable amplitude, we can understand that their action must always efface that of the configurative forces, and, on the other hand, the divisions formed in this manner being very long, we equally perceive, from what has been said above, that new configurative forces must be generated which would effect the separation; now, by reason of the resistance also indicated above, this separation should here yield but three parts at most, which, in view of the distribution of conflicts and concurrences, and the regulating action of the vibrations, must convert the sheaf into three jets only.

There remains, in the fourth place, the action of the sounds comprised between the grave octave and the fifth below, and between the latter and the double grave octave. For these sounds, there is no longer any simple relation between the lengths of the divisions which would result respectively from the vibrations alone and from the configurative forces alone; but it will be admitted without difficulty that, under the influence of those which approach, whether above or below, the double grave fifth, and in the case where the effect of the vibrations is not completely substituted for that of the configurative forces, the divisions due to these forces are a little shortened or elongated, so as to allow, at the limits which separate the successive systems of three of these divisions, the absolute concurrence of the two kinds of action, and thus to re-establish the simple ratio of 3 to 1 pertaining to the double fifth; whence the resolution into three jets. Under this same influence, as under that of the double fifth, if the vibrations are preponderant, but not sufficiently so to oppose an ulterior development of configurative forces, each large division can be but separated into two, so that the discontinuous part of the vein shall present but two jets. It will be also admitted that the sounds nearest to the grave octave will cause the mode related to this latter to prevail, and that in this case also the sheaf will never change except into two jets. As regards the sounds which do not depart too much from the double grave octave, the vibrations have always sufficient amplitude, and consequently sufficient action, to overpower the ordinary configurative forces, and at the same time the divisions to which they give rise are always sufficiently long to admit of each of them subsequently undergoing a separation, which divides it at most into three, and may also separate it into but two, if it encounters a greater resistance on the part of the vibrations; and hence two jets or three. As to the systems of expansions and nodes which are observed in each of the jets, they are plainly a consequence of the acquired transverse velocities which proceed from the action of the vibrations.

§ 29. It may be asked why, above the principal sound and between that and its grave octave, no sound, with the exception of those which approximate to these two last, had occasioned, in the experiments described in § 27, anything

analogous to the phenomena which we have just been considering? in effect, for the simple grave fifth of the principal sound, by way of example, it will readily be seen that the length occupied by the sum of two of the divisions due to the vibrations alone would be equal to that occupied by the sum of three divisions due to the configurative forces, so that by imagining these two sums superposed and combined, there would be a concurrence in the two constrictions of which the terminations of the system would constitute a part, and conflict in the two intermediate constrictious pertaining to the second of the two sums under consideration; and since these two conflicts would be equal, we might expect, agreeably to our theory, to see the sheaf give place to three jets; and we might also expect, for analogous reasons, the manifestation of three jets under the influence of the fourth sharp, and of two jets under that of the fifth sharp of the principal sound.

But, by our theory, the appearance of one, two, or three jets in place of the sheaf supposes, as we have seen to be the case, that the vibrations communicated to the liquid should regulate what passes in the vein, and this requires that they should have an energy of action capable of neutralizing the effect of the disturbing causes which tend to establish, in the successive constrictions as they arise, inequalities of length not symmetrically distributed; now, all things being otherwise equal, the action of the vibrations on the vein decreasing with the amplitude of these vibrations, we can conceive that above the grave octave of the principal sound this action may simply be insufficient, and that if it had been possible to augment, by a more immediate transmission or by a better disposition of the system of the orifice, the amplitude of the vibrations communicated, the three sounds indicated above would have ceased to show themselves inactive in regard to the sheaf. This will become evident, if we observe that the vibrations act on veins projected obliquely in the same manner as on veins directed vertically from above downwards, and if we recall that, in the experiments of Savart mentioned in No. 14, § 3, and explained in §§ 21 and 22, experiments in which everything was so arranged as to give great intensity to the vibrations communicated, the mode of transformation imparted by these last, was completely substituted for that of the configurative forces, even as regards sounds extending to the fifth sharp of the principal sound.

We have spoken of the possible influence of a change in the system of the orifice, and this because the orifice employed in my experiments was pierced in a very thin plate, (being but about half a millimetre in thickness,) and hence this plate vibrated, perhaps with difficulty, in unison with sounds not having a certain degree of gravity.

§ 30. We have now, in order to finish the theoretical examination of the influence exerted by vibratory movements on liquid veins, only to show the connexion of the theory with the facts of No. 17, of § 3.

Since the principal sound is also that (§§ 5, 12, and 26) for which the duration of a vibration is equal to the duration of a constriction or a dilatation at the contracted section, and since, from experiment, the number of vibrations corresponding to that sound proportionally diminishes as the direction in which the jet is thrown departs from the descending vertical, the same is necessarily the case with the number of incipient constrictions and dilatations, and consequently with the number of incipient divisions. But, as the velocity of discharge of the liquid is obviously independent of the direction of that discharge, the number of divisions which originate in a given time can only decrease notably by an augmentation in the length of these incipient divisions; hence, with the same discharge and the same orifice, the incipient divisions continue to lengthen in proportion as the direction of the emission of the vein departs more from the descending vertical. Now, this result is directly deducible from the hypothesis of § 2. In effect, while a vein directed vertically from above

downwards tends to grow more slender by reason of the acceleration of the movement due to gravity, a vein directed vertically from below upwards, on the contrary, tends to grow thicker on account of the retardation due to gravity; and since, according to the hypothesis in question, the progressive attenuation of the vein directed from above downwards occasions, by virtue of the reciprocal dependence *(solidarité)* of the divisions, a diminution of length in the incipient divisions, the thickening of the vein directed from below upwards must, for the same reason, occasion an augmentation of length in the incipient divisions; whence it follows that, when the direction of the emission of the vein passes progressively from the first of these cases to the second, the incipient divisions will continue gradually to grow longer.

As is seen by the number under discussion, from the direction of the descending vertical of the vein to the horizontal direction, the lowering of the principal sound is inconsiderable, but it becomes considerable from the horizontal direction to that of the ascending vertical, which implies that the same shall be the case with the lengthening of the incipient divisions. Now, this fact also flows from the hypothesis of § 2: in effect, the vertically ascending vein tends to be much more thickened, especially towards its upper extremity, on account of the gradual annulment of the velocity of the liquid, than the vertically descending vein tends to become slender at an equal distance from the contracted section; consequently, and still in virtue of the solidarity of the divisions, when the vein, thrown at first in the horizontal direction, continues approaching the ascending vertical direction, the successive augmentations in length of the incipient divisions must become much greater than when the vein, quitting the vertically descending direction, attains by degrees the horizontal direction.

The facts observed, being thus connected in a necessary manner with the hypothesis of § 2, serve reciprocally for confirmation of the latter, and it was to them that we had allusion when we said (§ 2) that this hypothesis was sustained by the results of experiment.

§ 31. In terminating the second series, we announced that in the present one, after completing what relates to liquid veins, we should treat of figures of equilibrium other than the sphere and cylinder, but in order not to give too much extension to this memoir, we have decided to reserve the latter subject for another occasion.

NOTE.—Since the publication of our theory of the constitution of liquid veins, as explained at the end of the previous series, the discussion of such veins has formed the subject of several successive publications, which we propose briefly to recall.

In 1849 M. Hagen presented to the Academy of Berlin a memoir *on the disks which are formed at the meeting of two liquid veins, and on the resolution of isolated liquid veins into drops*, (Poggendorff's *Annalen*, vol. lxxviii p. 451.) The experiments made by the author on isolated veins conduct him to a law, in regard to the relations between the length of the continuous part, the discharge and the diameter of the orifice, which does not seem to him to coincide with those of Savart. We are convinced that the disagreement is but apparent. In fact, Savart has only given his laws as approximative; and besides, as we have shown, (2d series, § 80,) these laws only constitute limits which the results of experiment approach the more closely as, for a definite orifice, the least of the discharges employed is stronger, and as, for a less but definite discharge, the orifice is smaller. As to the phenomenon of the resolution into isolated masses, M. Hagen, who could have no knowledge of our theory, the latter having been then too lately published, hazards the conjecture that this resolution is probably attributable to the capillary forces.

In 1851, M. Billet-Selis published, in the *Annales de Chimie et de Physique*, (t. xxxi, p. 326,) a notice *on the means of observing the constitution of liquid veins.* He there describes two different processes: the first is that which was indicated some time ago by myself for the observation of rapid periodical movements, the employment, namely, of a revolving disk pierced with narrow slits, equidistant and in the direction of the radii; the second, which is an ingenious modification of that of Savart, consists in producing, by help of a large concave mirror, a real and inverted image of the vein, under such an arrangement that the vein and its image shall appear confounded. I will recall, in this connexion, another process, communicated in 1846 to the Academy of Sciences at Paris by M. Matteucci, (*Comptes Rendus*, vol. xxii, p. 260,) which is a happy application of that devised by M. Wheatstone, for the case of rapid movements: it consists in illuminating the vein by a strong electric spark.

A memoir entitled *Nouvelle Theorie de l'Ecoulement des Liquides* was presented to the Academy of Sciences of Paris, (February 26, 1855,) by M. Dejean, but is generally known only by a short analysis, which we owe to the author himself, and which was inserted in the scientific journals; this treats, among other subjects, of the constitution of liqnid veins projected from circular orifices, and of the action exerted on them by vibratory movements. M. Dejean admits, for the case in which the vein is withdrawn from all extraneous action, the existence of the pulsations which Savart supposed to be produced at the orifice by the efflux itself, and he seeks to explain these pulsations, the laws relating to their number, and a part of the phenomena which depend on the influence of sounds. The analysis in question makes no mention of our theory.

Still another memoir, entitled *Recherches Hydrauliques*, was presented, about the same time, by M. Magnus, to the Academy of Berlin, (Poggendorff's *Annalen*, vol. xcv, p. 1.) The author occupies himself chiefly with the phenomena which are manifested when two veins meet under certain angles, and with the different aspects assumed by veins which issue from orifices of different forms; but he speaks also of the constitution of veins escaping from circular orifices and of the influence of sounds. M. Magnus, who likewise makes no mention of our theory, attributes the separation of the masses which compose the discontinuous part to the increasing inequality of the velocities of two contiguous horizontal strata of the liquid of the vein. As to the manner in which the sounds act, the little that he says reverts to the idea of Savart of which we have ourselves made use in the present series, that, namely, of successive compressions and tractions exerted by the vibrations, but he combines it with his own opinion on the formation of the discontinuous part.

Inasmuch as the theory which we have developed at the close of the second series is not based, as regards its fundamental principles, on hypothetical considerations, but is the necessary consequence of results of experiment; as it gives an explanation of all the details and of all the laws of the constitution of veins projected from circular orifices and not subjected to the influence of vibratory movements; as the present series, finally, renders equally an account of all the phenomena occasioned by this last influence, we have thought it useless to enter into any discussion in regard to the above theories.

FOURTH SERIES.

Figures of equilibrium of revolution, other than the sphere and the cylinder.

§ 1. The preceding series having completed the theoretical study of the liquid vein, we return to liquid masses withdrawn from the action of gravity, and propose to prosecute the examination of figures of equilibrium of revolution.

Let it be remembered, in the first place, that if we designate by R and R′ the two principal radii of curvature at the same point of the free surface of a liquid mass virtually without weight, and by C a constant, the expression of the general condition which such a surface should satisfy in a state of equilibrium is (2d series, § 5) $\frac{1}{R}+\frac{1}{R'}=C$, an expression in which R and R′ are positive when they pertain to convex curvatures, or, in other words, when they are directed to the interior of the mass, and negative in the opposite case; let it be also remembered that this equation is a simple transformation of that which implies that the pressure exerted by the liquid on itself, in virtue of the mutual attraction of its molecules, does not change from one point to another of the surface of the mass, *(ibid.;)* and be it remembered, lastly, that, according to a known property of surfaces of revolution, if the figure of equilibrium pertains to that class, one of the radii R and R′ is the radius of curvature of the meridian line at the point under consideration, and the other is the portion of the perpendicular comprised between the point in question and the axis of revolution, or, as may be expressed more simply, the perpendicular to that point.

In this case, that is, in the case of surfaces of revolution, the preceding expression, put in the differential form, is completely integrable by elliptical functions, so that the forms of the meridian lines may be deduced from it, and it is this which M. Beer has proposed to do in a recent memoir,* in which, for the second time, he has done me the honor of applying the calculus to the results of my experiments; and, besides this, a property discovered by M. Delaunay† by means of the calculus, and since demonstrated geometrically by M. Lamarle,‡ enables us to attain the same object without having recourse to elliptical functions. We shall speak, in a proper place, of these resources of analysis and geometry; but, in the present series, we purpose to arrive at the forms of the meridian lines, at all their modifications and all their details, by a reliance upon experiment and by availing ourselves of simple reasoning applied to the relation which the equation of equilibrium establishes between the radius of curvature and the perpendicular. Our undertaking, in which experiment and theory will proceed side by side, may thus serve as a verification of the latter.

To avoid all ambiguity, we will replace the letters R and R′ by the letters M and N, the first of which will be understood to designate that one of the two principal radii of curvature which pertains to the meridian line, and the second that which constitutes the normal or perpendicular; so that, as regards figures of revolution, the general equation of equilibrium will be, $\frac{1}{M}+\frac{1}{N}=C$.

§ 2. This notation being adopted, we shall proceed, first, to demonstrate that the sphere is the only figure of equilibrium of revolution whose meridian line meets the axis. To this we may add the plane, if we consider it as the limit of spheres, or as the surface generated by a right line perpendicular to the axis.

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* *Tractatus de Theoria Mathematica Phænomenorum in Liquidis Actioni Gravitatis Detractis Observatorum.* Bonn, 1857.

† *Sur la Surface de Revolution dont la Courbure Moyenne est Constante.* Journal de M. Liouville, 1841, t. vi, p. 309.

‡ *Theorie Geometrique des Rayons et Centres de Courbure.* Bulletin de l'Acad., 1857, 2d series.

Let us conceive a figure of equilibrium of revolution not being either a sphere or a plane, and whose meridian line meets the axis. I maintain, in the first place, that this line can attain the axis only perpendicularly. In effect, if it intersected it obliquely, or if it were a tangent to it, the perpendicular would be null at the point of intersection or of contact, and the quantity $\frac{1}{M}+\frac{1}{N}$ would become infinite at that point,* while it would be of finite value at neighboring

* There is one case, however, in which this reasoning would seem not to be applicable. We may conceive a curve such that, at the point where it meets the axis, the radius of curvature would be null, and that in the neighborhood of this point the radius of curvature and the perpendicular would be of opposite signs; then the quantity $\frac{1}{M}+\frac{1}{N}$ would constitute a difference, of which both terms would at once become infinite at the point situated on the axis, and it is not apparent, at the first glance, that this difference might not remain finite. We have to demonstrate, therefore, that the thing is impossible if the curve does not meet the axis normally. For that purpose, but only in this case, we shall be obliged to make use of the known expressions of the radius of curvature and of the perpendicular in functions of differential coefficients.

If we take the axis of revolution as axis of the abscissæ, we shall have p and q, respectively the differential coefficients of the first and second order of y relatively to x:

$$M=\frac{(1+p^2)^{\frac{3}{2}}}{q} \quad \ldots\ldots\ldots\ldots \quad (1)$$

$$N=y\,(1+p^2)^{\frac{1}{2}} \quad \ldots\ldots\ldots\ldots \quad (2)$$

whence we deduce, for the relation of the two terms of the first member of the equation of equilibrium:

$$\frac{\frac{1}{N}}{\frac{1}{M}}=\frac{1+p^2}{qy} \quad \ldots\ldots\ldots\ldots \quad (3)$$

Now, let $y=f(x)$ be the equation of the meridian line. Taking as origin of the co-ordinates the point where this line meets the axis, so that for $x=o$ we have $y=o$, we can then suppose the function $f(x)$ developed in a series of ascending and positive powers of x; and if we assume that the curve meets the axis under an angle other than a right angle, which requires that, for $x=o$, the first differential coefficient should be finite or null, it will be necessary that the exponent of x, in the first term of the series, should be unity. Let us remark here that, having only to consider the curve at the point where it reaches the axis and at points very near, we may always consider x extremely small, so that, in relation to this portion of the curve, our series will be necessarily convergent. Let us say, then:

$$y=ax+bx^m+cx^n+ \quad \ldots\ldots\ldots\ldots \quad (4)$$

an equation in which the exponents m n.... are positive and greater than unity. Consequently we shall have:

$$p=a+mbx^{m-1}+ncx^{n-1}+ \ldots\ldots\ldots\ldots$$

$$q=m\,(m-1)\,bx^{m-2}+n\,(n-1)\,cx^{n-2}+ \ldots\ldots\ldots\ldots$$

The first of these expressions, when we make therein $x=o$, is reduced to $p=a$, so that the curve meets the axis under a finite angle, but other than a right angle, or under an angle null, according as we suppose the constant a finite or equal to zero. Then, if we assume that at the point situated on the axis the radius of curvature is null, we see, by formula (1,) that in this same point q must be infinite, and, in virtue of the second of the above expressions, this condition will be satisfied if the first, at least, of the exponents m n.... be less than 2.

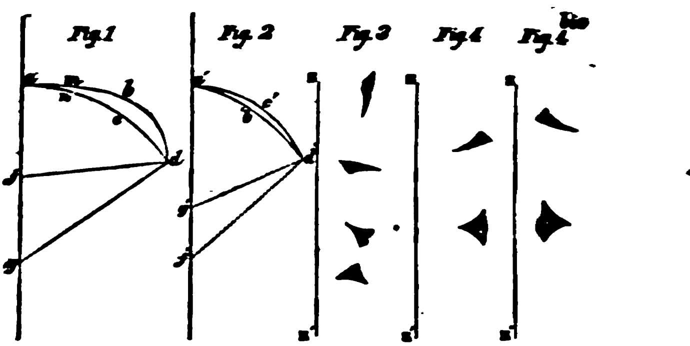
Let us now introduce into the formula (3) these same expressions of p and q and that of y. There will result:

$$\frac{\frac{1}{N}}{\frac{1}{M}}=\frac{1+(a+mbx^{m-1}+ncx^{n-1})^2}{(m\,(m-1)\,bx^{m-2}+n\,(n-1)\,cx^{n-2}+\ldots.)\,(ax+bx^m+cx^n+\ldots.)}$$

and we readily see that, for $x=o$, this ratio becomes infinite. Then, in effect, since the quantities m n.... are all greater than unity, on the one hand the numerator is reduced to $1+a^2$—that is to say, to a finite quantity; and, on the other hand, the denominator, of which the term of the smaller exponent, after the requisite multiplication, is $m\,(m-1)\,abx^{m-1}$, is

points; this quantity would, therefore, not be constant in the entire line of the curve as the equation of equilibrium requires.

Let us imagine, now, that the liquid fulfils the condition just laid down, and proceed to consider, at its departure from the axis, an arc of the meridian line. As, by the hypothesis, this line is neither straight nor circular, the curvature of the arc will vary from one point to another; it will commence, consequently, either by a process of augmentation or diminution, and we may take an arc so small that the curvature shall go on constantly augmenting or constantly diminishing from the point situated on the axis, quite to the other extremity. Let us suppose, first, the curvature continually increasing, and let $a\ b\ d$ (Fig. 1) be the arc in question. At the point a the perpendicular is coincident with the axis, and, in proportion as it leaves that point, forms with the axis a progressively greater angle; but we will so limit the length of the arc that, from a to d, this angle shall not cease to be an acute one. Through the two points, a and d, let us describe an arc of a circle, $a\ c\ d$, which shall have its centre on the axis, and which, consequently, meets this axis perpendicularly.



Since the arc $a\ b\ d$, whose curvature constantly increases, departs from a in the same direction with the arc of a circle, and, after being separated from it, rejoins it at d, it is evident that its curvature must, at first, be less than that of this second arc, and afterwards become greater, so that at the point d the radius of curvature of the arc $a\ b\ d$ is smaller than the radius of the arc of the circle. But from the common initial direction of the two arcs, and from this relative progression of the curvature of the arc $a\ b\ d$, it necessarily results that this last is, as the figure shows, exterior to the other, and that at the point d it must cut and not be a tangent to it; if, then, at this point d, we draw the perpendicular $d\ f$ to the arc of the curve and the radius $d\ g$ of the arc of the circle, the former will be less oblique to the axis than the latter, and will consequently be shorter. Thus, at the point d, the two quantities M and N will be both less than the radius of the arc of the circle. Let us take now, in the part of the arc $a\ b\ d$, where the curvature is less than that of the arc of the circle, any point m, and let us take, on the second of these arcs, a point n, so that the portion $a\ n$ shall be equal in length to the portion $a\ m$. Under these conditions, the point m will be evidently more remote from the axis than the point n,

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entirely annulled. We may remark, in passing, that this result is independent of the condition $m<2$, so that it is true as well for a radius of curvature, finite or infinite, at the point situated on the axis, as for a radius of curvature null, which should be the case according to what has been seen above. Now, if at this same point the radius of curvature is null, the two quantities $\frac{1}{N}$ and $\frac{1}{M}$, both assume, indeed, an infinite value; but, since their ratio becomes at the same time infinite, their difference becomes also infinite, which is what was required to be demonstrated.

and, on the other hand, the perpendicular at m will be more oblique to the axis than the radius drawn from n; for this double reason the perpendicular in question will be greater than the radius of the arc of the circle; but, because of the inferiority of the curvature at m, the radius of curvature at that point will be also greater than the radius of the arc of the circle.

From all this it results that the values of M and N, corresponding to the point m, are both superior to those which correspond to the point d; but it is clear that M and N are of the same sign throughout the length of the arc $a\ b\ d$, and that thus, at the point m as at the point d, the quantity $\frac{1}{M}+\frac{1}{N}$ constitutes a sum; this same quantity, then, is smaller at m than at d, and, consequently, the equilibrium of the liquid figure generated is impossible.

If we suppose, now, that the curvature of our meridian arc constantly diminishes, as is seen in $a'\ b'\ d'$, (Fig. 2,) it is apparent that then this arc will be interior to the arc of a circle $a'\ c'\ d'$, having its centre on the axis, that its curvature will at first be superior and become afterwards inferior to that of the latter, and that at the point d' one of the arcs will again intersect and not be a tangent to the other; whence we may conclude, by the mode of reasoning employed in the preceding case, that the quantity $\frac{1}{M}+\frac{1}{N}$ is greater at a point near a' than at d', so that the figure generated is, as before, impossible. Hence, when the meridian line meets the axis, the condition of equilibrium cannot be satisfied unless that line is a circumference of a circle, having its centre on the axis, or, if we suppose the radius of this circumference to be infinite, a right line perpendicular to the axis; the figure generated, therefore, is necessarily either a sphere or a plane.

From this flows, as a necessary consequence, the truth of the proposition which I advanced (2d series, § 28) from the results of experiment, namely, that when a continuous and finite portion of a surface of equilibrium rests on a circular periphery, that portion must constitute a spherical cap or a plane. To be otherwise, it would be necessary that the cap should not be a curve of revolution—a supposition which is never realized.

§ 3. The meridian lines of such other figures of equilibrium of revolution as can have no point in common with the axis, must either be extended infinitely, or be closed beyond the axis. The first class will generate figures which extend to infinity, and of these the cylinder has already afforded an example. The second would yield annular figures; and we shall see, at the end of the present series, whether the existence of figures of that kind is possible.

To simplify the investigation of the lines in question, we shall proceed to demonstrate that they contain no point of retrogression. If we suppose the existence of a point of that nature, there are three cases to be considered: first, that in which the tangent at the point of retrogression, a tangent which is common to the two branches of the curve, is not perpendicular to the axis of revolution, whatever direction it may otherwise have; second, that in which this common tangent is perpendicular to the axis, and where the two branches approach the latter in proceeding towards the point of retrogression; and, third, that in which the common tangent, being again perpendicular to the axis, the two branches, in proceeding towards the point of retrogression, withdraw from that axis.

First case.—By casting the eyes on Fig. 3, which represents, in meridian sections, portions of the liquid figure, for different positions of the point of retrogression in relation to the axis of revolution ZZ′, we readily perceive that in the neighborhood of that point the perpendicular is always, as regards one of the branches, directed to the interior of the liquid, and is consequently positive;

while, as regards the other, it is directed to the exterior, and is consequently negative. Now, the equation $\frac{1}{M}+\frac{1}{N}=C$ cannot comprise this change of sign of the perpendicular N in passing from one branch to the other, for it would require that at the point of retrogression this perpendicular should be null or infinite; and in the present case the perpendicular in question is evidently finite, since the tangent is not perpendicular to the axis, and the point of retrogression cannot be upon the latter.

Second case.—If the point of retrogression be of the second kind—that is to say, if the two branches which meet therein are situated on the same side of the common tangent—we see that, for one of these branches, the perpendicular and the radius of curvature are both positive, while for the other they are both negative; the quantity $\frac{1}{M}+\frac{1}{N}$ would then change the sign in passing from one to the other, and thus would not be the same through the whole extent of the liquid figure.

If the point of retrogression is of the first kind—that is, if the two branches are situated on the two opposite sides of the common tangent—the radius of curvature, we know, is there null or infinite, but a radius of curvature null would render infinite the quantity $\frac{1}{M}+\frac{1}{N}$, so that we have to examine only the hypothesis of a radius of infinite curvature. Since, then, from the direction of the tangent, the perpendicular is also infinite at the point which we are considering, the quantity $\frac{1}{M}+\frac{1}{N}$ would be reduced to zero at the same point; it would therefore be necessary, for equilibrium, that this quantity should also be null at all other points of the meridian line. Now, this is impossible, since, when we depart from the point of retrogression, the radius of the curvature and the perpendicular assume, on each of the branches respectively, values finite and of the same sign.

Third case.—If the point of retrogression is of the second kind, the radius of curvature has opposite signs on the two branches, and consequently must be either null or infinite at the point in question; but, as has been already shown, we need not occupy ourselves with the hypothesis of a radius of curvature null; there remains, therefore, that of a radius of curvature infinite. Now, the perpendicular at the same point being likewise infinite, equilibrium requires, as above that the quantity $\frac{1}{M}+\frac{1}{N}$ should be null for all the points of the meridional line. Here, at first glance, the thing seems possible, since, near the point of retrogression, the radius of curvature and the perpendicular, on each branch considered separately, are of contrary signs, but we shall presently see that this possibility is but apparent.

If the point of retrogression be of the first kind, the radius of curvature is there necessarily null or infinite, as has been already shown; and since we must reject the radii of curvature null, the quantity $\frac{1}{M}+\frac{1}{N}$ is again equal to zero at the point in question, and must be so likewise at all other points; which appears possible as in the former case, and for the same reason. But in order that at all points of the meridian line the quantity $\frac{1}{M}+\frac{1}{N}$ should be null, it is evidently necessary that in each of these points the radius of curvature must be equal and opposite to the perpendicular. Now geometers are aware that one curve alone possesses this property, and that that curve is the catena, *(chainette,)* which has no point of retrogression.

§ 4. The principles established in the two preceding paragraphs having eliminated from the question of our meridian lines the complications which might have embarrassed it, we may proceed to deal more directly with the subject.

In the experiments relative to the formation of the liquid cylinder between two solid rings (2d series, § 38,) when the upper ring has been raised so as to cause the mass of oil to lose its spherical form, but not sufficiently to cause it to assume the cylindrical form, we obtained a portion of a figure of equilibrium of revolution pertaining neither to the sphere nor to the cylinder; it was shown, moreover, that if, after having formed the cylinder, the separation of the rings increased, there would result another portion of a figure of equilibrium equally differing from the sphere and the cylinder, and which will of course be understood as being also one of revolution. In order to determine what the liquid figures, to which the portions in question pertain, would be in their completed state, let us first cite a new experiment.

We take as a solid system a cylinder of iron of considerable length in proportion to the diameter, and supported by two feet made of wire of the same metal (Fig. 5;) let the length, for instance, be 14 centimetres, and the diameter 2. This cylinder being carefully rubbed with oil and introduced into the vase, we bring into contact with it, midway its length, a sphere of oil of suitable volume. As soon as adhesion takes place, the liquid mass spreads upon the surface of the cylinder so as to envelop a part of its extent, loses the spherical form, and constitutes in the end a figure of revolution whose meridian line changes curvature in the direction of its two extremities, becoming at those two points a tangent to the generating cylinder.* The meridian section of the liquid figure and of the cylinder is represented at Fig. 6.

Fig. 5

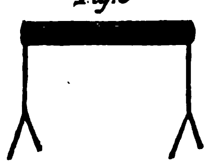


Fig. 6

§ 5. As we have shown theoretically, (2d series, §§ 6 *bis*, 10, 18 and 20,) and have verified by many experiments, when the liquid mass adheres to a solid system which causes it to lose its spherical form, the only parts of that system on which the new figure of equilibrium depends are the very minute lines along which it is in contact with the superficial layer or stratum of the mass, so that, the system may in general be reduced to iron wires representing those lines. Now, in the figure which we are considering, the free surface of the liquid mass touches our solid cylinder along two circumferences perpendicular to the axis, and passing by the points a and b; we may therefore readily conceive the entire cylinder replaced by two rings representing those circumferences, that is, with an exterior diameter equal to that of the cylinder, and placed vertically as regards one another, having between them the interval $a\ b$. It will be necessary, however, that the quantity of oil should be greater in order to supply the volume of that portion of the cylinder suppressed in the interior of the mass; it will require even a little oil in excess to furnish the substance of the two bases which rest upon the rings, bases whose surfaces, as we shall presently see, will be convex spherical caps. In order to avoid these last, which would needlessly complicate the figure, we may take disks instead of rings; then, in both cases, the figure will be entirely formed of oil, and it is represented in this state, in

* M. Beer (see note 1 of § 1) indicates the same experiment for verifying one of the results of his calculations; but I had employed it long before.

its meridian section or vertical projection, by Fig. 7, *a m* and *b n* being the sections or projections of the disks. It will be stated hereafter for what reasons we have suggested the use of a cylinder rather than of disks or rings.

Fig.7

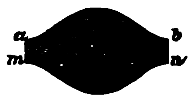
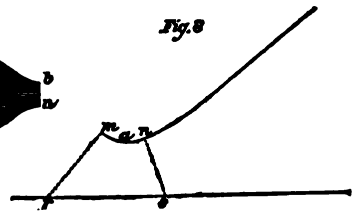


Fig.8



§ 6. The figure which we have thus obtained, and in which the meridian line stops at the points *a* and *b* where it touches the cylinder (Fig. 6) or meets the borders of the disks (Fig. 7,) evidently constitutes but a portion of the complete figure of equilibrium. Let us attempt then to follow the meridian line, starting from these same points *a* and *b* where its elements are parallel to the axis.

It is easy to show that the points *a* and *b* are not points of inflexion. At such points the radius of curvature is either null or infinite; but since, in our meridian lines, there can be no question of a radius of curvature null, which would render the first member of the equation of equilibrium infinite, it would be necessary to suppose this radius infinite at the points which we are considering, and the equation would there be reduced to $\frac{1}{N}=C$. Now, the points *c* and *d* (Fig. 6) are really points of inflexion of this kind, as the aspect of the figure shows, in so much that the equation of equilibrium is there necessarily reduced to $\frac{1}{N}=C$; the perpendicular N should then, at the points *a* and *b*, have the same length as at the points *c* and *d*, which is evidently not the case; for, in the first place, the points *c* and *d* are more remote from the axis than the points *a* and *b*, and, moreover, the perpendiculars which proceed from the former are oblique to the axis, while those which correspond to the latter are perpendicular to it.

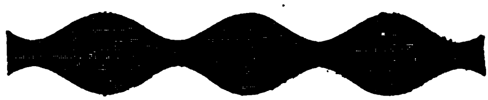
Beyond the points *a* and *b*, then, the curve begins by preserving a curvature having the same direction as before, that is, a curvature concave towards the exterior (Fig. 8.) Now, let us suppose that in the prolongation starting from *a*, for instance, this curvature should continue either augmenting or diminishing less than it diminishes on the other side of *a*; we can always take on the prolongation in question a portion *a m* so small that at each point the curvature shall be stronger than at the corresponding points of a portion *a n* of the same length taken on the first part of the curve. By virtue of the greater curvature of all the points of the arc *a m*, the point *m* is necessarily more remote from the axis than the point *n*, and, moreover, the perpendicular *m r* which proceeds from the former is more oblique to the axis than the perpendicular *n s* which proceeds from the second; the perpendicular at *m* is, for this double reason, greater than the perpendicular at *n*. On the other hand, conformably with the same hypothesis relative to curves, the radius of curvature at *m* is smaller than at *n*. Thence it results that in passing from the point *n* to the point *m*, the first term of the quantity $\frac{1}{M}+\frac{1}{N}$ will increase and the second diminish. Now, in the parts of the curve which we are considering, the radius of curvature and the perpendicular are opposite to one another, and have consequently contrary signs, so that the quantit[illegible] $\frac{1}{\ }\ \frac{1}{\ }$ [illegible]onstitutes a difference; if, then, one of the terms of this

quantity increases while the other decreases, it cannot preserve the same value, and equilibrium is impossible. If we suppose, on the contrary, that the curvature of the arc *a m*, on parting from *a*, diminishes more than that of the arc *a n*, we shall conclude, by the same mode of reasoning, that the quantity $\frac{1}{M}+\frac{1}{N}$ would likewise change its value in passing from one of the parts of the curve to the other.

Thus the hypothesis of curvatures either greater or less in the arc *a m* than in the arc *a n* is incompatible with the equation of equilibrium; it is consequently necessary, in order to satisfy this equation, that, on the small prolongation *a m*, the curvatures should be identically the same as on an arc *a n* of the same length taken on the other side of *a*. Now, it is clear that this implies the identity of the whole portion of the curve situated beyond the point *a* with the portion situated within it. The portion of the curve comprised between *a* and *b* (Figs. 6 and 7) will be reproduced, therefore, beyond *a*, and, for the same reasons, will be still reproduced indefinitely; and the same will be the case on the other side of the point *b*, in such manner that the meridian line will represent an undulating curve extending to infinity along the axis, approaching and retiring alternately and periodically by equal quantities.

The complete figure of equilibrium, therefore, is prolonged to infinity along the axis, and is composed of a regular and equal succession of expanded and constricted portions, of which Fig. 9 represents a meridian section of a certain

Fig. 9



extent. To this figure of equilibrium we shall apply, in the sequel, the name of *unduloid*, from the form of its meridian line.

§ 7. It is easy to conceive how equilibrium may exist in such a figure, although in the dilated parts the curvature is convex in all directions around the same point, while, in the constricted parts, the curvature is convex in certain directions and concave in others: it is because, in these latter parts, the convex or positive curvatures are stronger than the concave or negative curvatures, so that the mean at each point (2d series, § 6) is positive and equal to that which corresponds to the different points of the dilated parts. From the fact that, in the unduloid, the mean curvature is positive, it necessarily results that whenever we realize any portion of an unduloid between two rings, the bases which rest upon the latter will be convex spherical caps.

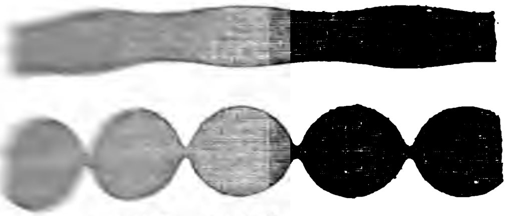
§ 8. If, in the experiment of § 4, the volume of oil remaining the same, we employ a solid cylinder of greater diameter, the liquid mass extends still more in the direction of the axis, and the meridian curvatures diminish, so that, in the corresponding complete figure, the expansion and constrictions are less decided. Thus the meridian curvatures, in the partial and consequently in the complete figure, become proportionably effaced as the diameter of the solid cylinder is greater; whence we perceive that, in these variations, the complete figure tends towards the cylindrical form, which may be considered, therefore, as the limit of the variations.

If, on the contrary, while the volume of oil still remains the same, we employ a solid cylinder of smaller diameter, the liquid mass becomes constricted in the direction of the axis, the meridian curvatures augment, and the figure approximates more and more to the sphere; thus, for instance, when for a mass of oil constituting primarily a sphere 6 millimetres in diameter, we take, as solid cylin-

... in thickness, the mass assumes almost exactly ... if we use a very fine wire, the variance from the spheri- ... imperceptible. And inasmuch as the complete figure ... in all its parts, the dilatations and constrictions will be ... at the final limit, the figure will consist of a suc- ... tangent to one another on the axis.

... therefore, vary in form between two very wide ... the cylinder, and, on the other, a succession of ... one another on the axis. In Fig. 10 are represented

Fig.10



... which differs little from the cylinder, and the other ap- ... spheres. In these different aspects the figure of equi- ... occupied has, as we see, an analogy with the succes- ... formation of an indefinite liquid cylinder (Fig. 30 of 2d

... the unduloid is susceptible of another kind of variation, which ... us suppose a vase similar to that we have been using, ... greater; let us place therein horizontally, immersed in ... solid cylinder 2 centimetres in diameter, for instance, and ... supported on feet sufficiently elevated. We cause to ... a mass of oil which shall produce a portion of an undu- ... Fig. 6, and then add a new quantity of oil; the figure ... length and at the same time in thickness; but let us push ... so that one of its extremities shall be brought back to ... first, and the other only remain extended. If we add ... quantities of oil, still pressing back the first extremity of the ... this figure will progressively acquire greater thickness, ... will retire more and more; and, as we may conceive ... the cylinder as long as we please, there is nothing ... term to the theoretical possibility of the increase of the ... as well as length. If, then, we suppose this increase carried ... of the convex meridian arc and the second extremity of ... no longer, so that the meridian line, beginning with the ... will continue to retire indefinitely from the axis; and since the ... mentioned constitutes the neck *(cercle de gorge)* of a constricted ... on both sides of such a constriction, everything is perfectly ... (§ 6) we perceive that the complete meridian line will be reduced ... curve with two infinite branches, like the parabola, having its axis perpendicular to the axis of revolution; consequently the com- ... generated will itself be reduced to a single constriction, extending ... from one part to the other of its *cercle de gorge*. We shall pre- ... in a precise manner, the nature of this third limit of the unduloid.

§ 19. Let us return, now, to the employment of two disks for the realization of the portion of unduloid comprised between the middle points of two neighboring constrictions (§ 5). When we attempt this realization by attaching to the two disks a ... than should constitute the figure, and then

gradually absorb the excess by means of the small syringe, the operation proceeds without difficulty so long as the elements of the meridian line which terminate at the edges of the disks deviate considerably from parallelism with the axis; but when they approximate to this parallelism, or, in other words, when we approach the portion of the unduloid which we wish to obtain, it is necessary to operate with greater precaution, as the figure might otherwise change spontaneously and disunite. By conducting the operation with care, and, towards the end, removing the oil only in very small quantities, we arrive, as far as the eye can judge, at the desired portion of the unduloid, (Fig. 7,) a portion which varies in form by approaching or withdrawing from the cylinder, according as the diameter of the disks is greater or smaller relatively to their distance; but then the slightest cause, such as a minute movement communicated to the mass by the point of the syringe, is sufficient to produce the gradual alteration and destruction of the figure, which is seen to grow progressively thinner near one of the disks, the oil being transferred in greater quantity to the side of the other disk, and the mass finally separates into two parts. From the fact that, in the figure obtained as above, an alteration, occasioned by the most trifling cause, proceeds afterwards spontaneously, it would seem that the portion of unduloid comprised between the middle of one constricted portion and that of the next is at the limit of stability.

We see, from what has just been said, why, in § 4, the adoption of a cylinder as a solid system was recommended. With disks, there is need of the greatest circumspection and care to arrive at the point where the last elements of the meridian line are or appear parallel to the axis; while with the cylinder the figure is perfectly stable, and the required parallelism is established of itself. But it remains to be explained how the stability of the figure can depend on the two circumferences along which the superficial stratum of the mass touches the cylinder, (§ 5.) This is easily done: in the case of the disks, when it happens, as has been said, that the figure grows thin spontaneously on one side, the elements of the superficial stratum which terminate at the edge of the disk near which this effect takes place are inclined towards the axis, (Fig. 11;) but, in the case of the cylinder, the last elements of the superficial stratum cannot be thus inclined, since they lie on the surface of the solid.

Fig. 11

Fig. 12

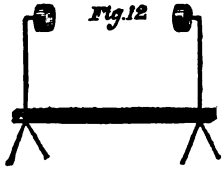
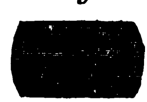


Fig. 13



This explanation naturally suggests the idea of substituting for thin disks thick ones, or, rather, portions of a cylinder; for, by giving to the mass, at first, a sufficient volume for the oil to reach the edges of the faces of these thick disks opposite to those which front one another, and then removing so much of the liquid that the circumferences of contact shall fall on the thickness of the disks, the cause of stability, above indicated, will evidently exist just as well as with a continuous cylinder. Now, this is fully confirmed by experiment; the disks which I used had each a diameter of 15 millimetres and a thickness of 8, and were fixed at a distance of 90 millimetres apart; the entire system is represented at Fig. 12. By causing to adhere to the whole a mass of oil, at first too great, then removing the excess, and lightly pressing the mass to right or left with the point of the syringe, so that the points from which the meridian line appeared to depart were nearly at an equal distance from the two bases of each disk, the figure produced evinced a perfect stability; it is practicable, by

continuing to absorb small quantities of oil, to bring the extremities of the meridian line very near the edges of the solid bases fronting one another without a loss of stability in the figure, and only when they seemed to reach those edges was instability manifested.

§ 11. Since the portion of unduloid with which we are occupied has already reached the limit of stability when it is formed between two thin disks, and is thus free in its whole extent with the exception alone of its bases, it would be useless to seek to realize a portion of unduloid equally free which should extend on both sides beyond the centres of two constrictions, and hence we infer that the indefinite unduloid is, like the indefinite cylinder, an unstable figure of equilibrium. An experiment, however, of our 2d series, affords incidentally an unduloid which is prolonged beyond the centres of two constrictions, but very close to the cylinder; to this we shall return hereafter.

§ 12. It is now easy to see that the convex figures spoken of in § 38 of our 2d series, while describing the formation of the liquid cylinder, figures which are obtained when, after having attached a sphere of oil to two horizontal solid rings equal in diameter and placed one above the other, we raise the upper ring by a less quantity than that which gives to the mass the cylindrical form—that these figures, I say, are nothing else but portions of the dilatations of the unduloid; only, when these convex figures are produced by the process just recalled, they are so placed that their axis is vertical.*

Let us conceive, in effect, an unduloid realized by means of two thick disks, (§ 10,) and consequently in a state of stable equilibrium, and imagine that we place at equal distances to the right and left of the middle of this figure, between that middle and the thick disks, two vertical solid rings, having their centres on the axis and their exterior circumference precisely at the surface of the mass; it is clear that these rings will not destroy the equilibrium of the figure. Now, if we suppose that the parts of the figure situated beyond these rings are replaced by convex spherical caps resting on the latter, and whose curvature is such that it occasions a pressure equal to that which pertains to the rest of the figure, equilibrium will still evidently exist, and it will still be perfectly stable, since the distance of the rings is less than that which corresponds to the limit of stability. But, then, if the rings are not sufficiently separated for the portion of the meridian line which extends from one to the other to contain points of inflection, it is evident that the whole will constitute one of the convex figures in question; for, according to the different forms of the unduloid, the meridian line of the portion comprised between the rings may vary from an arc of a circle, with its centre on the axis, to a straight line, as in these convex figures. For these last not to be portions of an unduloid, it would be necessary that between the same rings, placed at the same distance from one another, and with an equal mass of oil, there should be two figures of equilibrium possible, both of them stable, which experiment contradicts. If, after having transformed a sphere of oil into one of the convex figures in question, whether by increasing the separation of the rings or by subtracting a certain quantity of the liquid, we agitate the alcoholic mixture so as to give considerable motion to the mass of oil, but still not enough to disunite it, and then allow it to return to a state of rest, it will always resume identically the same form.

In the experiments of §§ 44 and 45 of the 2d series, when the rings or disks were placed at a distance of four times their diameter, and the liquid mass comprised between them was sufficient for the stability of the figure, this figure evidently constituted part of an unduloid; but as, by the abstraction of oil, we afterwards arrived, through a very small diminution of the mass, at its spontaneous destruction, it follows that the portion of unduloid in question was but

* One of these convex figures is represented at Fig. 21 of 2d series.

little removed from its limit of stability, and that hence its meridian line contained very probably points of inflection.

In describing the experiments of § 65 of the same series, experiments which commence with the momentary realization of a cylinder a little transcending the limit of stability, it was said that the spontaneous alteration of this cylinder was sometimes manifested by the formation of two constricted portions comprising between them one dilated portion; that this state of the figure, after attaining no very decided development, appeared to remain stationary for some time; that then one of the constricted portions was slowly obliterated while the other deepened, and the transformation continued afterwards in the ordinary manner. Now, from the fact that this figure, with two contractions, persists for a considerable time, it must be inferred that it constitutes a figure of equilibrium, and consequently an unduloid little different from the cylinder and surpassing the limit of stability—that is, extending itself beyond the centres of the two constricted portions. In effect, since such an unduloid, although unstable, is a figure of equilibrium equally with the unstable cylinder, it may likewise be formed, for some moments, between the disks, and it may be conceived that a slight accidental cause would suffice to transform the mass from one of these figures into the other. We see, finally, that, in the experiments of § 10 of the present series, the liquid mass thus always constitutes a portion of an unduloid which becomes modified, without ceasing to pertain to this kind of figure, in proportion as we absorb the excess of oil.

§ 13. The transient unduloid, spoken of above, verifies the conclusions of § 6 relative to the pursuit of the meridian line beyond points of the concave parts where the elements become parallel to the axis. Unfortunately this unduloid is not produced at will; its meridian curvatures are weak, and it is otherwise unstable; but another experiment, to which allusion has been made without describing its results, furnishes a precise verification of the same conclusions.

If, after having formed between two rings a vertical cylinder whose height is much less than that which would correspond to the limit of stability, we slightly raise the upper ring, the cylinder is observed to become somewhat hollowed in the meridian direction, so that the figure presents a constriction; if the ring be again raised, the constriction still deepens and the figure remains perfectly symmetrical on both sides of the *cercle de gorge*, which is, consequently, situated at the middle of the interval between the rings. If, in the cylinder with which we started, the ratio between the height and diameter was suitable, we may, by proceeding thus, render the constriction very decided, and then the meridian line changes the direction of its curvature by tending towards the rings, so that it presents two points of inflection at an equal distance on both sides of the *cercle de gorge;* the bases of the figure, also, preserve their convex form, and even their curvature increases more or less. In this experiment there is always, we may conceive, a limit to the separation of the rings, beyond which equilibrium is no longer possible; if we overpass it, the constricted portion grows spontaneously more slender, till it breaks and the figure separates into two portions; but, for every degree of distance less than the limit in question, the equilibrium is stable. The cylinder which has appeared to give the above results most distinctly, is that whose height is to the diameter nearly in the ratio of 5 to 7. In employing, for instance, rings of 70 millimetres in diameter, it is proper to form a cylinder of about 50 millimetres in height; the upper ring may then be raised until it is distant from the other nearly 110 millimetres, and we thus obtain a figure in which the *cercle de gorge* has but a diameter of some 30 millimetres.

The experiment thus executed requires great precaution: the equality in the densities of the two liquids and the homogeneity of the oil should be perfect, and when the limit of separation of the rings is approached, it is necessary to proceed with much circumspection. But we succeed without difficulty by so

arranging that the axis of revolution shall be horizontal; the rings of 70 millimetres, which are then vertical, should be previously placed at a distance of 110 millimetres apart; each of them is attached, by its lower part, to a vertical iron wire, and the wires are themselves fixed, at their lower extremities, in a plane table of iron, which supports the whole system; finally, these wires are enveloped with cotton, that the oil may not adhere to them, (2d series, § 9.) A cylinder (Fig. 13) is first formed between the two rings, then we gradually diminish the volume of the mass by means of the small syringe. If, when the neck is not more than about 30 millimetres in diameter, we take care to remove the oil by only very small portions at a time, we shall succeed in reducing it to 27 millimetres, and thus obtain the result represented by Fig. 14.

Now, it is evident that all these constricted figures with convex bases—figures which, like those considered in preceding paragraphs, may deviate as little from the cylinder as we choose—are still portions of unduloid, though taken differently in the indefinite unduloid: while the middle of the one is occupied by the equator of a dilated portion, the middle of the others is occupied by the *cercle de gorge* of a constricted portion; the most extended of the former, except the transient unduloid mentioned above, is composed of an entire dilatation between two demi-constrictions, (Figs. 6 and 7,) and that represented by Fig. 14 is composed of an entire constriction between portions of two dilatations.

Fig. 14

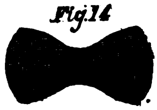
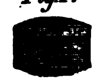


Fig. 17



§ 14. Resuming, now, our horizontal rings, with a view of placing, at will, the upper one nearer to or further from the other, let us again form a cylinder between them, and, without changing their distance, gradually remove oil from the mass. If the ratio of the distance of the rings to their diameter is much less than in the last experiment of the preceding paragraph, the curvature of the bases, instead of augmenting in proportion as the constriction deepens, continues, on the contrary, to diminish; and if this ratio does not exceed about $\frac{2}{3}$, the bases at length become absolutely plane. With a ratio still less, we may even proceed further; if the absorption of liquid is continued the bases become concave. Let us form, for instance, between our rings of 70 millimetres diameter, a cylinder 35 millimetres in height, (Fig. 17;) by gradual absorption of the oil, we shall see the bases sink more and more at the same time that the constriction grows deeper, and, their curvature at length wholly vanishing, we shall have the result represented by Fig. 18. If we still continued to use the syringe, the bases would assume a concave curvature; but let us pause, for the moment, while they are yet plane.

Fig. 19

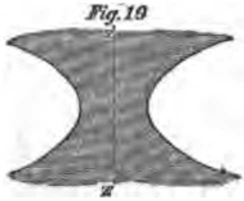
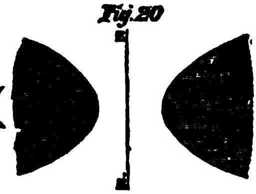


Fig. 18

Fig. 20



With such bases, the constriction comprised between the rings can no longer (§ 7) pertain to the unduloid, and we arrive, consequently, at a new figure of revolution. Let us inquire what this is, in its complete state. We remember (2d series, § 4) that the pressure corresponding to an element of the superficial

stratum has for its value $P+\frac{A}{2}\left(\frac{1}{R}+\frac{1}{R'}\right)$, an expression in which A is a constant dependent on the nature of the liquid and which cannot be null, and P the pressure corresponding to a plane surface. Now, in the case with which we are occupied, the pressure at any point of the complete figure must be equal to that of a plane surface, since the bases of our partial figure are planes; the above expression then will, in this case, be reduced to P, so that we have $\frac{1}{R}+\frac{1}{R'}=0$. Thus the figure in question is such that at each point of its surface the mean curvature (2d series, §§ 5 and 6) is null, or, in other terms, at each of these points there are, as in the portion formed between our rings, concave curvatures whose effect exactly destroys that of the convex curvatures, so that the pressure remains the same as if there had been no curvature.

Now, the equation $\frac{1}{R}+\frac{1}{R'}=0$ becoming here, according to the notation which we have adopted for figures of revolution, $\frac{1}{M}+\frac{1}{N}=0$, we deduce therefrom $M=-N$; whence we see that, at each point of the meridian line, the radius of curvature is equal and opposed to the perpendicular. Now, geometers have demonstrated that the only curve which possesses this property is the catena, *(chainette.)** This, then, is so placed relatively to the axis to which the perpendiculars are referred, that the right line, which divides it symmetrically into two equal parts, shall be perpendicular to that axis, and the summit of the curve distant from the point of intersection of those two right lines by a quantity equal to the radius of curvature of that summit. Our figure, then, in its complete state, is that which would be generated by the revolution of a catena thus placed in relation to the axis. We will, accordingly, give it the name of *catenoid*, of which Fig. 19 represents a meridian section sufficiently extended, the axis of revolution being ZZ'.

The catena being a curve, whose branches are infinite, the catenoid also is extended to infinity, like the cylinder and the unduloid, but no longer in the direction only of the axis.

§ 15. We recall here a principle which was cursorily noticed in § 8 of the 2d series, and of which we afterwards made use in § 31 of the same series: when a surface satisfies the general condition of equilibrium of our liquid figures, that condition is equally fulfilled whether we suppose the liquid on one or the other side of the surface in question. In effect, the inversion of the position of the liquid, with regard to the surface, only changes the signs of the two principal radii of curvature corresponding to each of the points of the latter, but evidently does not at all alter the absolute values of those radii, so that if the quantity $\frac{1}{R}+\frac{1}{R'}$ is constant in one of the cases, it will be so in the other. There are always, then, for any one surface which satisfies the condition of equilibrium, two liquid figures, the second of which presents in concave what the other presents in relief, and, *vice versa*, figures which are both figures of equilibrium. We see this realized, for instance, in our experiments as regards the sphere; a mass of oil left free to itself in the midst of the alcoholic mixture gives a sphere in relief, and, on the other hand, when some of the alcoholic mixture is introduced into one of our masses of oil, the surfaces into which the bubbles of this mixture are moulded constitute spheres of oil in concave, (2d series, § 10.) In virtue of this principle we have two catenoids; that, namely,

* The catena will be recognized as the curve formed, in a state of equilibrium, by a heavy and perfectly flexible chain suspended at two fixed points.

of Fig. 19, in which the liquid fills the space left by the catena in revolving between itself and the axis, and another in which the liquid occupies the space embraced by the curve. A meridian section of the latter is represented by Fig. 20.

§ 16. In the experiment of § 14 we only succeed, as has been said, in rendering the bases of the figure plane when the separation of the rings does not exceed about $\frac{2}{3}$ of their diameter. We shall recur, further on, to the details of this experiment, which presents some curious particulars; but there is an important consequence which is deduced immediately from it, and which requires our notice at present; for rings of a given diameter there is a maximum of separation beyond which no portion of a catenoid is any longer possible between them. We shall proceed to show that this result is in accordance with the theory, and we shall, at the same time, be conducted to a new result.

We have seen that the generating catena should satisfy the condition that the radius of curvature of its summit be equal and opposite to the right line which measures the distance of that summit from the axis of revolution. This being so, let us conceive, in a meridian plane, a right line perpendicular to the axis of revolution, and representing the axis of symmetry of the catena, and again a second right line parallel to the axis of revolution and distant from the latter by a quantity equal to the radius of the rings. Let us conceive, further, in the same plane, a generating catena having its summit at the point where the right line of the rings is intersected by the axis of symmetry of which we have spoken. This catena will be tangent at that point to the right line in question, and consequently cannot rest upon the rings except when the peripheries of these pass by the point of tangence, or, in other words, when the mutual distance of the two rings shall be null;* the catena under consideration corresponds then to the case of a separation null of the rings. Let us now suppose that the curve quits this position and proceeds gradually towards the axis of revolution, being so modified as always to satisfy the condition of equality between the radius of curvature of its summit and the distance of that summit from the axis; in each of its new positions it will cut the right line of the rings at two points, which we will designate as A and B. The distance of these two points will then represent, in each of these positions, the distance apart of the rings, and the corresponding catena will represent the meridian line of a catenoid, of which the rings would comprise a portion between them. This being premised, let us consider the evolutions of the points A and B. In the initial position of the catena, when its summit is tangent to the right line of the rings, these points are confounded at the point of tangence; but when the summit of the curve begins to advance towards the axis of revolution, they separate and progressively remove from one another. But their mutual distance will attain a maximum, after which that distance will continue to diminish. In effect, conformably with the condition attached to the catena, when its summit shall have arrived very near the axis of revolution, the radius of curvature of that summit will have become very small; whence it follows that the two branches of the curve will closely approach one another, and that, consequently, the two points A and B will also be in close proximity; finally, when the summit is on the axis, these same points will be again reunited, as then the radius of curvature of the summit will be null, and the two branches of the curve will form but a single right line coincident with the axis of symmetry. Thus the points A and B, which were first coincident and then diverged from one another, afterwards approach, until at last they again coincide; from which it necessarily follows, as just stated, that their mutual distance attains a maximum; and it is easy to see, from the nature of the curve, that this maximum

* For simplicity, we here consider the two rings as possessing no thickness.

must be finite, and indeed cannot be considerable relatively to the diameter of the rings.*

It is evident that, in its transit to the axis of revolution, the curve has passed through all the conditions which, with the given rings, are consistent with equilibrium; the above maximum, then, constitutes a limit of separation for the rings, beyond which there can be no catenoid between them.

But the preceding furnishes another consequence equally remarkable. Since, during the transit of the summit of the catena, the points A and B first withdraw from and afterwards again approach one another, they necessarily repass by the same distances, so that, for each distance less than the limit, they pertain at once to two catenæ. Now, it results from this that, to every degree of separation less than the maximum, there always correspond two distinct catenoids resting on these rings, but penetrating unequally between them. We see without difficulty that the summits of the two generating catenæ, summits which, for a separation null, are the one at the common periphery of the rings in contact, and the other on the axis of revolution, approach one another more and more in proportion as the separation increases, and finally coincide, equally with the two entire curves, when that separation attains its maximum. Thus the two catenoids will differ so much the less as the separation of the rings is greater, and when the limit has been reached will form but one.

§ 17. All catenæ are, we know, alike; and hence if we imagine a series of complete catenoids generated by catenæ of different dimensions, all these catenæ, from the condition which they must satisfy, (§ 14,) will be similarly placed in relation to the axis of revolution, and consequently all the catenoids will be similar figures. The complete catenoid, then, is not susceptible of variations of form like the unduloid, but constitutes an unique figure, like the sphere and the cylinder. Hence the two complete catenoids which, theoretically, rest on the same rings, when the separation of these is below the limit, do not differ from one another except by their dimensions absolutely homologous.

§ 18. Of the two partial catenoids pertaining to these two complete catenoids, and equally possible by the theory between the rings, our process necessarily gives that which is least re-entering; if we attempt, by removing further quantities of oil from the mass, to realize the most deeply re-entering catenoid, there is always, as we shall presently see, another figure of equilibrium produced. From the impossibility, therefore, of realizing this partial and most deeply re-entering catenoid, we may justly conclude that it would constitute an unstable figure of equilibrium.

As to that which is least re-entering, it evidently forms a portion of the complete catenoid, so much the more extended as the separation of the rings is nearer its maximum; for, in proportion as the rings are more widely separated, the arc of the catena which they intercept between them is (§ 16) a more considerable portion of the curve. In order to have a partial catenoid more extended in relation to the complete catenoid, it would be necessary that the catena should penetrate more deeply between the rings; but then, by however small a quantity the summit of the curve should advance, the separation of the rings would diminish, (*ibid.*,) there would be another catena possible, less re-entering and resting on the same rings, and the partial catenoid generated by the first catena being the most re-entering, it would be unstable. The catenoid of greatest height constitutes, then, the most extended portion of the complete catenoid which can be realized between two equal rings.

We will notice here another consequence to which the above would seem to lead, and which would yet be opposed to fact; for every degree of separation

* We can determine its precise value by means of the equation of generating catenæ, but this calculation is reserved for the series in which we shall unite all the applications of mathematical analysis with the subject of our researches.

less than the maximum, the catenoid which is least re-entering always proves perfectly stable, and, as has been shown above, that which is most re-entering must be regarded as always unstable. Now, the catenoid of greatest height forms, as has also been seen, the transition between the catenoids of the first category and those of the second, and consequently between stable and unstable catenoids; we might, therefore, take it for granted that the catenoid of greatest height is at the limit of stability of that kind of figure; and yet, when we realize it with a mass of oil, it manifests a decided instability. We shall presently know to what this apparent contradiction is attributable.

§ 19. It is readily seen that the third limit of the variations of the unduloid, a limit spoken of in § 9, is nothing else but the catenoid. In effect, by causing the partial unduloid to vary in the manner indicated in that paragraph, it is clear that, in proportion as the volume of the mass is increased, the perpendicular and the radius of curvature relative to the summit of the convex meridian arc continue to increase and become infinite at the same time with the volume; whence it follows that at that limit the quantity $\frac{1}{M}+\frac{1}{N}$ is null, and this we know to be the character of the catenoid.

The quantity $\frac{1}{M}+\frac{1}{N}$, or, what is the same thing, $\frac{1}{R}+\frac{1}{R'}$, tending thus towards zero in proportion as the unduloid approaches the catenoid, the general expression (§ 14) of the pressure exerted by an element of the superficial stratum shows that this pressure tends, at the same time, towards that of a plane surface. If, then, we imagine between two rings a constricted portion pertaining to an unduloid, and that this unduloid is tending, by degrees, towards a catenoid, the bases of the figure, bases whose pressure must always be equal to that of the constricted portion, will necessarily become less and less convex, and be finally altogether plane. Now, this is what is evidently realized by the experiment of § 14; when, after having formed between two rings a cylinder whose height does not exceed $\frac{2}{3}$ of the diameter, we gradually withdraw liquid from it and the bases sink, by degrees, till they lose all curvature, the constriction which is produced and which deepens in the same proportion pertains to an unduloid which is tending towards its third limit, and thus the experiment in question exhibits before our eyes the progressive transition of the unduloid into the catenoid. If we collate the preceding with the contents of § 13, we shall be authorized to deduce the conclusion that every constriction, resting on two rings and presenting convex bases, is a constriction of an unduloid, whether the curvature of the bases be superior, equal or inferior, to that of the bases of the cylinder which would be comprised between the same rings.

§ 20. We will recite, now, the circumstances which have been presented to us by the experimental investigation of the partial catenoid of greatest height.

The diameter of the rings employed was 71 millimetres. In all the experiments which follow, the process commenced with forming a cylinder, and then oil was withdrawn from the mass, at first by the syringeful and afterwards by small portions; from time to time the operation was suspended in order to observe the figure.

First experiment.—Distance of the rings 55 millimetres. The versed sine of the spherical caps, which constitute the bases, is gradually reduced to a fraction of a millimetre; then, during an interruption of the exhaustion, a singular phenomenon is produced; the figure undergoes a slight spontaneous modification; the convexity of the bases rapidly augments until the versed sine retrieves a value of about 2.5 millimetres, and consequently the constriction formed between the rings becomes somewhat thinner, and then the whole remains stationary. By still cautiously absorbing oil, the versed sine increases

to nearly 3 millimetres; finally, in consequence of a new absorption, the figure disunites in the usual manner at the middle of the constricted portion.

Second experiment.—Distance of the rings 49 millimetres. The bases present, in the end, a total loss of curvature, and then, as above, there is a spontaneous transformation: the bases again become slightly convex, with a versed sine of about 1 millimetre. A new absorption brings on disunion.

Third experiment.—Distance of the rings 47 millimetres. The bases again appear to become plane, and the figure continues in this state. Further absorptions seem, at first, to have no other effect than to deepen the constricted portion, while the bases still appear plane; then a slight convexity is re-established, but not now spontaneously; it originates and increases in correspondence with the exhaustion; when the versed sine is about 1.5 millimetre, disunion takes place.

Fourth experiment.—Distance of the rings 45 millimetres. The bases become first plane, then slightly concave. The versed sine of this concavity increases nearly to 2 millimetres, and again a spontaneous transformation is observed; the concavity is changed into a convexity, whose versed sine is nearly a millimetre. The action of the syringe then occasions disunion.

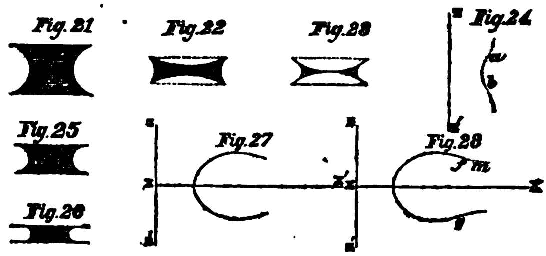
Fifth experiment.—Distance of the rings 43 millimetres. The bases are rendered plane, then concave, and the versed sine of the concavity gradually attains 4 or 5 millimetres; the figure then disunites.

§ 21. Let us consider what these experiments teach; first remarking that it is not easy to judge of the precise point at which the bases of the figures are rendered plane, for an exceedingly slight curvature eludes the sight. Hence arises some uncertainty in the determination of the limit of height of the catenoid; fortunately the particulars which we have noticed will furnish us a means of appreciation more exact.

In the fourth experiment we necessarily realize plane bases, from the circumstance that the curvature, from being convex, becomes gradually concave by the progressive absorption of the liquid; but is this the case likewise in the second and third, in which we seemed also to have realized planes? This is a point which we will attempt to elucidate. The first, second, and third experiments have this in common, that a small spontaneous modification or transformation of the figure is produced therein, while in the third this phenomenon does not occur; and this modification is observed decreasing from the first to the second, disappearing in the third, and reappearing in the fourth. From this we should infer that the third experiment forms a sort of transition, on one and the other side of which the spontaneous transformations are manifested; but the effect was shown in the first experiment when the bases had still a visible curvature, and in the fourth when they had assumed one in an inverse direction; it is highly probable, then, that in the second, at the moment when the spontaneous transformation was seen to occur, the bases still preserved a real curvature, though too feeble to be distinguished; and that it was only in the third, where the distance of the rings was 47 millimetres, that bases entirely plane were attained. If, in this third experiment, the bases conceived to be plane seemed not to begin to lose this state until after the absorption of a very considerable quantity of liquid, that evidently results from the difficulty mentioned above of clearly distinguishing the point at which the curvature is annulled.

Thus, for our rings of 71 millimetres diameter, we may admit that the distance of 47 millimetres differs very little from that at which we begin to obtain bases strictly plane; and since 47 is obviously $\frac{2}{3}$ of 71, we may conclude that

the maximum height of the partial catenoid is, either exactly or very nearly, $\frac{2}{3}$ of the diameter of the bases. This catenoid is represented by Fig. 21.



Let us now call attention to the slight spontaneous transformations, considered in themselves. Till now, when we saw one of our liquid figures become transformed, and thus pass from an unstable to a stable equilibrium, the alteration was profound, the mass separated into two or several parts, and the final result of the phenomenon always consisted of spheres or portions of spheres. Here, there is nothing of the kind: the alteration is inconsiderable; the mass does not disunite, and the final result is a figure which deviates little from the former, at least in the portion realized, and which may be of the same nature. In the first experiment, for example, an unstable partial unduloid is transformed into another unduloid but little different, and doubtless the same is the case in the second. Moreover, what is still more remarkable, the comparison of the first two experiments seems to indicate that the unstable unduloid and the stable unduloid into which it is converted approach one another indefinitely in proportion as the distance of the rings is nearer the maximum height of the catenoid.

The experiments which we are discussing furnish the key of the difficulty indicated at the end of § 18 in regard to the stability of the partial catenoid of greatest height. When, the rings being at the distance which corresponds to this catenoid and a cylinder formed between them, the small syringe is put in operation, the figure becomes, as we know, unduloid, which, varying with the progress of the absorption, tends towards the catenoid; but the third experiment further shows that if, after having attained that limit, we continue the operation, the figure again insensibly becomes an unduloid which deviates, in proportion to the exhaustion, from this same catenoid. If, then, the partial catenoid of greatest height constitutes the transition between partial catenoids stable and partial catenoids unstable, it constitutes, on the other hand, the transition between a continuous series of stable unduloids and another continuous series of unduloids equally stable. Such is evidently the reason of the decided stability of the partial catenoid of greatest height; hence, when, by means which will be explained in a subsequent series, we render impossible the formation of every other figure but the catenoid, this loses its stability as soon as we give it the maximum height.

We close here the study of the unduloid and catenoid and pass to that of a third figure.

§ 22. Of this third figure we already know a portion: it is the constriction with concave bases obtained in the last two experiments of § 20, a constriction which, by the nature of those bases, is foreign to the unduloid and catenoid. To realize it, it is requisite, as has been seen, that the distance of the rings should be less than $\frac{2}{3}$ of the diameter; Fig. 22 represents, in its meridian section, such a constriction, for a distance of the rings equal to about a third of the diameter, and when the bases have already become strongly concave; the dotted lines are sections of the planes of the rings. Let us now endeavor, as in

the case of the two preceding figures, to determine the complete form of the meridian line.

We will mention first a remarkable transformation which the partial figure undergoes when the ratio between the distance and the diameter of the rings is sufficiently below $\frac{2}{3}$ to allow the abstraction of a large quantity of liquid without occasioning disunion, and we carry this abstraction as far as possible. The constricted portion and the bases alike becoming more concave, we know there must arrive a moment after which their surfaces can no longer co-exist without mutually cutting one another; there is then produced a phenomenon of the same nature as with the liquid polyhedrons, (2d series, §§ 31 to 35)—that is to say, the figure passes gradually to a laminar state: two conical films are seen to form, proceeding respectively from each of the rings, and at the centre of the system a plane film, such as is shown in meridian section at Fig. 23. These films acquiring more and more development in proportion to the continued absorption of oil, the whole tends finally to be reduced to a sort of double laminar and truncated cone; but one of the films always breaks before we can reach that point. It hence results that if we wish to observe the constriction in all its phases with the form proper to it as pertaining to the new figure of equilibrium, it is necessary to oppose an obstacle to the generation of films. Now this is accomplished without difficulty by substituting disks for rings, and thus preventing the bases from becoming concave; we may then remove oil until the figure spontaneously disunites at the middle of its height.

§ 23. Before pursuing the meridian line beyond the limits of the partial figure, we should offer two important remarks.

In the first place, the constricted portion, whether realized between rings or disks, always shows itself perfectly symmetrical on both sides of the *cercle de gorge*. This is equally required by the theory, for the mode of reasoning of § 6 is independent of the nature of the meridian line, and applies as well to the constricted portion with which we are occupied as to that of the unduloid. If, then, in a meridian plane, we imagine a right line perpendicular to the axis of revolution and passing by the centre of the *cercle de gorge*, all that the complete meridian line presents on one side of the above right line, it will also present, in a manner exactly symmetrical, on the other side, so that this right line will constitute an axis of symmetry.

In the second place, since, by employing rings, the bases of the partial figure are concave, it follows that, through the whole extent of the complete figure the pressure is less than that of a plane surface. Now, agreeably to the formula of such pressure, (§ 14,) this requires that the quantity $\frac{1}{R}+\frac{1}{R'}$, or, according to the notation adopted in this series, $\frac{1}{M}+\frac{1}{N}$, should be finite and negative. In our new figure, therefore, the mean curvature (2d series, §§ 5 and 6) is negative—that is to say, at each point of this figure concave curvatures predominate.

§ 24. The points *a* and *b*, (Fig. 22,) at which the partial meridian line stops, cannot, in the complete meridian line, be points of inflexion. We see, in fact, from the direction of the tangent at those points, that if the meridian line, at its departure thence, pursued a curvature in the contrary direction, (Fig. 24,) the radius of curvature would, in this part of the figure, be directed to the interior of the liquid like the perpendicular, and that thus the quantity $\frac{1}{M}+\frac{1}{N}$ would become positive; which cannot be, by reason of what has been said above.

Beyond the points *a* and *b*, then, the meridian line begins with a concave curvature; and the same direction of curvature is evidently maintained, for the same reason, so long as the curve continues to retire at once from the axis of revolution and the axis of symmetry. But the curve cannot continue to sepa-

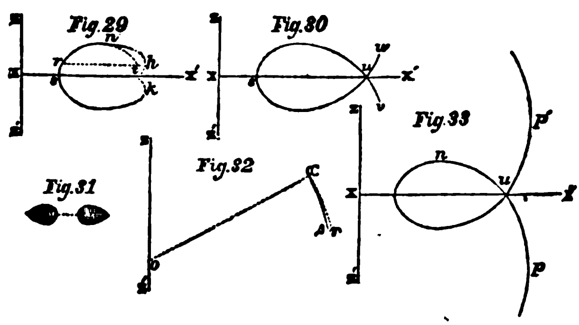
rate indefinitely from those two axes: in effect, if such were its course, it is clear that the curvature must diminish so as to be annulled, in each of the two branches, at the point situated at infinity, whence at that point the radius of curvature would have an infinite value; and as it would evidently be the same as regards the perpendicular, the quantity $\frac{1}{M}+\frac{1}{N}$ would become null at that limit. It necessarily follows that at a finite distance from its summit the curve has two points in which its elements are parallel to the axis of symmetry, and this experiment confirms, as we are about to see.

§ 25. If we use disks, which are placed at a distance equal to about the third of their diameter, and carry the absorption of liquid sufficiently far, the angle comprised between the last elements of the surface of the mass and the plane of each of the disks diminishes until completley annulled, so that that surface is then tangent to the planes of the disks, (Fig. 25,) and hence the last elements of the meridian line are parallel to the axis of symmetry. It is very difficult to judge of the precise point where this result is attained, but we ascertain that it is really produced by continuing the exhaustion of the liquid: we soon see the circumferences which terminate the surface of the mass abandon the margins of the disks, withdraw, by a diminution of diameter, to a certain distance within them, and leave a small zone of each of the solid planes free; now, as these zones remain necessarily moistened with oil, though the stratum be excessively thin, it is clear that the surface of the mass must there meet the planes tangentially. If the separation of the disks is still less, we obtain a result of the same nature; only, before spontaneous disunion takes place in the middle of the figure, we may still further contract the circumferences of contact, or, in other words, enlarge the extent of the free zones.

§ 26. The reason assigned in § 24 to establish the absence of an inversion of curvature so long as the curve withdraws at once from the axis of revolution and the axis of symmetry, evidently still holds good at the points which we have just been considering, that is to say, at those where the elements are parallel to this last axis; whence it follows that the curve afterwards approaches this latter axis, by preserving the same direction of curvature, as is shown at Fig. 27, where the curve is drawn on a larger scale than the portion comprised in Fig. 25, and where the axis of symmetry is represented by the right line XX′. And so long as these prolongations of the curve continue to withdraw from the axis of revolution, the direction of the curvature must still remain the same. For let us suppose that it changes, at f and at g for instance, (Fig. 28,) then, from the point f to a point such as m, situated a little beyond, the radius of curvature and the perpendicular would have, it will be seen, opposite directions, so that the quantity $\frac{1}{M}+\frac{1}{N}$ would be a difference; now, from f to m the perpendicular would evidently go on increasing, since, on one hand, the distance from the axis of revolution increases, and, on the other, that perpendicular would have a still greater and greater obliquity; it would, therefore, be necessary, in order for the above difference to remain constant, that the radius of curvature should also continue to increase from f to m; but this is precisely the contrary of what would occur, for, by reason of the inflexion, the radius of curvature would be infinite at f, and consequently could only diminish after leaving that point. It is needless to remark that what has been just said applies equally to the point g.

Let us see now whether, before reaching the axis of symmetry, the curve can present two points, such as h and k, (Fig. 29,) where its elements shall be perpendicular to that axis. With that view we will examine what conditions the curvature should satisfy from the summit s to the points h and k, and it will suffice to consider the arc snh. Let n be the point where the element of

the curve is parallel to the axis of symmetry. From *s* to *n* the radius of curvature and the perpendicular have evidently contrary directions, and the quantity $\frac{1}{M}+\frac{1}{N}$ constitutes a difference; therefore, from one point to another of this arc, the quantities M and N must vary in the same direction; and as the perpendicular continues to increase from the point *s* to the point *n*, the radius of curvature must continue likewise to increase; whence it follows that from *s* to *n* the curvature is continually decreasing. Still further on, that is from *n* to *h*, we see that the radius of curvature and the perpendicular are directed towards the same side, so that the two terms of the quantity $\frac{1}{M}+\frac{1}{N}$ are of the same size, and hence from one point to another the quantities M and N must vary in opposite directions. Now, when we remove from *n* on the arc *nh*, the perpendicular begins to diminish, since at the point *n* it is infinite; while the radius of curvature begins to increase, or, in other terms, the curvature at the beginning diminishes, and, whatever its ulterior course, will always be, at every point of the arc *nh*, weaker than at *n*, for at all those points the perpendicular is finite, and consequently less than at *n*. But we know that the curvature continues to increase from *n* to *s*; therefore, in the whole extent of the arc *nh*, the curvature is less than at any point of the arc *ns*.



This being premised, let us draw the right line *hr* parallel to the axis of symmetry, and then construct, beginning at the point *n*, an arc *nt* exactly symmetrical as regards the arc *nr*. In the whole length of the arc *nh* the curvature, by reason of what has been said, will be less than at any of the points of the arc *nt*; whence it follows that this last arc will be entirely interior to the former. Now, the arc *nt* meets at *t* the right line *hr* by an element which necessarily makes with the part *tr* of that line an acute angle; then, in order that the arc *nh*, which proceeds from *n* in the same direction with the arc *nt*, should meet perpendicularly at *h* the right line *hr*, it would be necessary that, after separating from the arc *nt*, it should afterwards again approach it, which is evidently impossible in consequence of the inferiority of the curvature at all its points. We perceive, indeed, that it ought to cut the right line *hr* under a more acute angle than does the arc *nt*. Thus, the curve, in declining at its departure from *n* towards the axis of symmetry, cannot cease to withdraw from the axis of revolution; and since, moreover, it cannot change the direction of its curvature, it must necessarily intersect the axis of symmetry. We further perceive that, in consequence of the condition which governs its curvatures, it must cut that axis obliquely, so that we arrive, in the end, at the conclusion that it forms a node, (Fig. 30.)

We shall verify the existence of this node by means of experiment. If we have not commenced by doing so, it is because it was necessary first to demonstrate that, starting from a constriction, for which the pressure is less than for a plane surface, there is no other form possible for the meridian line.

§ 27. The constrictions realized in the experiments of § 25 being generated by a portion of the node of the complete meridian line, it is obvious that the figure generated by the entire node, from the summit of the latter to its point, would be concave in the interior of the oil; but it is indifferent, we know, (§ 15,) as regards equilibrium, whether the liquid be situated on one or the other side of the surface; the figure generated by the node may, therefore, be equally well supposed full or in relief, and it is in the latter state that our experiment will realize it. Only when the liquid is transported to that side of the curve, the quantities M and N at once change their sign, and consequently the quantity $\frac{1}{M}+\frac{1}{N}$ from being negative, as it was previously, becomes positive.

We form, in a ring of iron wire, a bi-convex liquid lens, (2d series, § 18,) whose thickness shall be about equal to the sixth of the diameter: for instance, with a ring 70 millimetres in diameter, the thickness of the lens should be about 12 mm. If we pierce perpendicularly this lens in its centre, by means which will be indicated below, we obtain a regular annular figure, limited externally by the solid ring, and continuing for two or three seconds; after which, the central opening is seen to stretch towards a point of the solid ring, the mass disunites at that point, and all the liquid flows towards the opposite part of the ring, there to form a large and perceptibly spherical mass. Now, the momentary annular figure, which is formed under these circumstances, is, though unstable, a figure of equilibrium, since it subsists for some moments, and its duration is long enough to enable us to observe that its meridian section has the form represented by Fig. 31, in which the dotted line is the section of the plane of the ring. This meridian section shows evidently that the surface of the figure produced is generated by a node having its summit turned towards the axis of revolution and its point to the solid ring.

Let us dwell for an instant on the details of the experiment just described and on certain modifications of it. To pierce the lens, we should employ a small cylinder of wood pointed at one end and joined at the other to an iron wire, which is bent obliquely, so that, holding it with the hand, we can introduce the small cylinder into the vase and pierce the lens perpendicularly. If the diameter of the solid ring be 70 mm., as we supposed above, that of the small cylinder should be about 16 mm.; and the cylinder and its point should be covered with cotton cloth in order to prevent all adhesion of the oil.

If we give the lens a thickness sensibly exceeding the sixth part of the diameter of the solid ring, the liquid returns upon itself as soon as the cylinder is withdrawn, and the mass resumes its lenticular form; but we may give a less thickness than the above limit, when the central opening will assume larger dimensions, and the node of the meridian line be consequently smaller. When the thickness of the lens is sufficiently inferior to the limit in question, the manner in which the spontaneous destruction of the unstable figure takes place is not the same; the central opening does not then extend towards a point of the solid ring, but the annular liquid mass contracts and disunites in several places at once, so as to be converted into a series of small isolated masses, which adhere to different parts of the metallic ring. The unstable liquid ring spoken of in § 19 of the second series pertains to the sort of figure which we are now studying, and it will be remembered that it proceeds from a lens whose thickness has been rendered as small as possible.

§ 28. As the liquid ring may thus assume, in the same solid ring, very different dimensions according to the thickness of the lens, or, in other words, according to the volume of the liquid of which it is formed, it results that, for the same distance from the point of the node of the meridian line to the axis of revolution, the length of the node may vary between wide limits: in the experiments above described, these variations are comprised between a very small

fraction of the distance in question and nearly three-fourths of that distance. The complete figure with which we are occupied is thus not always similar to itself, as are the sphere, the cylinder, or the catenoid; like the unduloid it is susceptible of variations of form. A comparison of the liquid figures represented by Figs. 25 and 26 leads to the same conclusion.

§ 29. Before proceeding, we will notice a remarkable particular. If we suppose the node in relief, the liquid which occupies it is in the concavity of the curve; and since this line does not change the direction of its curvature in passing the point u, (Fig. 30,) the liquid will still occupy the concavity of each of the prolongations uv and uw; it fills therefore the spaces comprised between these prolongations and the node, so that this node is engaged, whether completely or partially, in the interior of the mass. If we suppose the node hollow, (*en creux*,) it is, as may be easily seen, the prolongations uv and uw which are then engaged in the liquid. Hence results this singular consequence, that, though the general condition of equilibrium is satisfied, we cannot represent to ourselves the complete figure, except in the state of a simple surface, not in that of a liquid mass. In this last state it is only possible to imagine isolated portions of the figure—such, for instance, as the portion generated by the node alone. This peculiarity of a surface re-entering into the mass is one of those to which allusion was made in § 1 of the second series, and which would render the realization of certain figures of equilibrium in their whole extent impossible, even if those figures did not extend to infinity.

§ 30. Let us attempt now to discover the course of the curve beyond the points v and w, (Fig. 30.) We already know, from reasons stated in § 26, and illustrated by Fig. 28, that as long as the branches of the curve continue to retire from the axis of revolution, the curvature cannot change its direction, and consequently remains concave towards that axis. This being so, there are evidently but three hypotheses possible: either the branches in question retire from the axis of revolution in such manner that their distance from the latter tends towards infinity, or they tend towards an asymptote parallel to this axis; or each of them presents, at a finite distance from the point u of the node, (Fig. 30,) a point at which the element is parallel to the same axis. The first of these we may at once dismiss; it would require, as has been already shown, (§ 24,) that at the points situated at infinity on the two branches, the radius of curvature and the perpendicular should be both infinite, and thus the quantity $\frac{1}{M}+\frac{1}{N}$ would be equal to zero.

Let us examine, then, the second hypothesis, that, namely, of an asymptote parallel to the axis of revolution. At the point u (Fig. 30) the perpendicular is infinite, and the radius of curvature finite, (§ 26;) at the point where the branch uv prolonged would reach the asymptote, on the contrary, the radius of curvature would be infinite, and the perpendicular, which would measure the distance from that point to the axis, would be finite. In passing, then, from the point u to this extreme point, the perpendicular, at first superior in length to the radius of curvature, would afterwards become inferior to it; whence it follows that there would be on the curve a point where the perpendicular and the radius of curvature would be equal, and for which consequently the centre of curvature would be on the axis of revolution. Let a be this point, o the corresponding centre of curvature, and $a\beta$ a small arc of a circle described from the point o as a centre. One branch of the curve would quit the point a in the same direction and with the same curvature as the arc $a\beta$, and would then immediately separate from the latter. Now let us suppose that at its departure from a, the curvature should at first go on decreasing; the curve will, at commencing, be necessarily exterior to the arc of a circle. Let $a\gamma$ be a small arc of this curve, in the whole extent of which the curvature decreases, and let the length of the arc $a\beta$ be taken

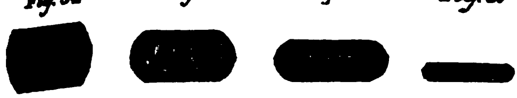
equal to that of the arc $\alpha\gamma$. The point γ will be more remote from the axis than the point β; and moreover, on account of the inferiority of the curvatures, the tangent at γ will form, with this axis, a greater angle than the tangent at β; the perpendicular, therefore, at the point γ will, for this double reason, be longer than the perpendicular at the point β. Again, and still by reason of the inferiority of curvatures, the radius of curvature at the point γ will also be longer than the radius of curvature at the point β; but, at this latter point, these two quantities have the same value as at the point a. In passing then from a to γ, the radius of curvature and the perpendicular will both increase. Now this is incompatible with the equation of equilibrium; as the curve, throughout the part which we are considering, turns its concavity towards the axis, the radius of curvature and the perpendicular have everywhere the same sign, and consequently when one increases the other should diminish, and *vice versâ*. If we suppose that, at parting from a, the curvature goes on increasing, the arc of the curve will be interior to the arc of a circle, and the same mode of reasoning would enable us to see that from one to the other extremity of the former the radius of curvature and the perpendicular will both diminish. The hypothesis of an asymptote parallel to the axis of revolution leading thus to an impossible result, we see that it must be rejected like the first.

It is the third hypothesis, therefore, which is true; that is to say, the curve presents two points, p and p', (Fig. 33,) where the tangent is parallel to the axis of revolution.

§ 31. Experiment fully confirms this theoretical deduction, and furnishes, besides, a suggestion for the discovery of the ulterior course of the curve.

The two disks being placed at any distance from one another—a distance, for instance, equal to their diameter—we form a cylinder between them, and then gradually lower the upper disk: the figure then passes, we know, to the unduloid, and swells more and more till it constitutes portion of a sphere, (Fig. 34.) But if we continue to lower the upper disk, the meridian convexity still augments, and consequently passes beyond the above point; for a certain approximation of the disks, we thus obtain, for example, the result represented by Fig. 35, and the liquid figure is always perfectly stable. Now, in this state, it can no longer form part of an unduloid, since the sphere has been exceeded, which is one of the limits of the variations of that figure, (§ 8.) We may again lower the disk until, at the points where the meridian line reaches the borders of the disks, the tangents shall be nearly perpendicular to the axis of revolution, as is seen in Fig. 36, and for a less mass of oil in Fig. 37. It is even possible that perpendicularity may be attained; but it would be very difficult to acquire the assurance of this, because, on the one hand, the eye cannot judge with sufficient precision of the direction of these extreme tangents, and, on the other, the liquid figure, at this degree of approximation of the disks, loses its stability; if we depress a little too much the upper disk, the oil is observed to be transferred in greater mass to one side of the axis of the system, so that the figure ceases to be one of revolution; then, on this same side, the oil overflows the borders of the disks, and spreads in part on their exterior faces.

Fig. 34 Fig. 35 Fig. 36 Fig. 37



Now, in virtue of what has been stated in the preceding paragraph, so long as the curve, at parting from n, (Fig. 33,) continues to withdraw from the axis of revolution, the radius of curvature cannot become equal to the perpendicular, and since it is infe[illegible] n, must remain inferior to it so long as the point

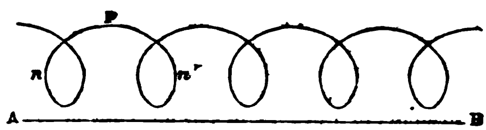
p is not attained; in the whole extent, then, of the arc *n u p*, except at the point *n*, and perhaps at the point *p*, to which the demonstration does not extend, the centre of curvature is always situated between the curve and the axis, and consequently the curvature is always stronger than that of a circumference of a circle having its centre on the axis. But, as we have just seen, in the partial liquid figures represented by Figs. 35, 36, and 37, the meridian curvatures are stronger than when the figure is a portion of a sphere, or, in other words, stronger than that of a circumference of a circle passing by the borders of the disks and having its centre on the axis. From this it is clear that these partial figures constitute portions of the complete figure generated by an arc of the meridian line extending on both sides of the point *p*, (Fig. 33;) only they relate evidently to different cases of that complete figure which we know to be susceptible of variations like the unduloid.

§ 32. We will take one more step in pursuit of our meridian line. In the above experiments, when the densities of the two liquids are rendered quite equal, the oil figure is always perfectly symmetrical in relation to its equatorial circle. It is by the eye, indeed, that we thus judge, and it might be supposed, perhaps, that this symmetry is but approximate; but we shall proceed to show that it is exact. In the absence of all accidental cause of irregularity, there would be evidently no reason why an excess of curvatures should exist rather on one definite side of the equator than the other, since the two disks are equal and parallel; whence it results that there is necessarily a form of equilibrium in which the symmetry is perfect. But if, in the partial figures realized—figures which are stable—symmetry were but approximated, it would be necessary to admit that the exactly symmetrical form of equilibrium just spoken of would be unstable. If, then, all the liquid figures which can be obtained in the experiments described above, that is to say, in those which give all the degrees of depression of the disk from the case of Fig. 34 to that of Fig. 36, and all the masses greater and smaller with the same disks—if, I say, all these figures were symmetrical only in appearance, there would correspond to each of them another figure of equilibrium differing extremely little, and which would be unstable. Now, the existence of two partial figures of equilibrium extremely near, the one stable and the other unstable, may well occur in a particular case of the variations of two complete figures, or, at least, of one of them, and we have seen an example (§§ 20 and 21) in regard to the contraction of an unduloid, when that contraction closely approximates the partial catenoid of greatest height; but we can comprehend that it is impossible for the same thing to be reproduced in the whole extent of the variations of the partial figure realized. Hence we conclude that, in the liquid figures of the preceding paragraphs, the symmetry is real, and that, in one complete meridian line, there is thus, besides the axis of symmetry of the node, another axis of symmetry equally perpendicular to the axis of revolution, and passing by the point *p*, (Fig. 33.) Consequently, all that the curve presents on one side of this point, it should present symmetrically on the other; the node which exists above *p* must have its corresponding node below, and since the two have respectively their axis of symmetry, it necessarily results that, in the first place, they are perfectly identical, and, in the second place, that all that is found on one side of one of them must be identically reproduced on the other side; whence it follows that above the upper node there is another like it, and above the last still another, and so on indefinitely along the axis of revolution, while the same thing occurs below the inferior node, all being connected by arcs equally identical with one another. An extended portion of this curve is represented at Fig. 38, in which the axis of revolution A B is placed horizontally.

The figure generated by this curve is thus prolonged indefinitely in the direction of the axis, like the cylinder and unduloid. We will give to this also a name, and will call it the *nodoid*. It should be observed that this figure being,

equally with the unduloid, susceptible of variations between certain limits, Fig. 38 should be regarded only as presenting one case of its meridian line. We

Fig. 38



will further recall the observation made in § 29, and which will now be better understood from the appearance of this curve, namely, that the complete figure can only be represented in the state of a simple surface, since, if it were supposed to be full, there would evidently be parts of it engaged in the mass.

§ 33. Before we proceed to the consideration of the nodoid in its variations, a question should be resolved which is suggested by the experiments of § 31. Since we know now the form of the meridian line, we see that those experiments realize the portion of the nodoid generated by a part, more or less considerable, of one of the arcs convex towards the exterior, such as *npn'*, (Fig. 38.) But it may be asked if this does not require that, with disks of a given diameter, the volume of oil should be comprised within certain limits, so that for larger or smaller volumes the figure realized would no longer pertain to the nodoid. To determine this, let us take one of the figures realized, follow the meridian arc beyond the point where it meets the edge of one of the disks—the upper one, for instance—and let us see whether it be possible to arrive at a curve other than the meridian line of a nodoid.

We will suppose, first, that in that part of its course where it continues to approach the axis of revolution, and to withdraw from the axis of symmetry, the curve presents a point of inflexion, so that it shall afterwards turn its convexity towards those two axes. If, while still approaching the first, it changed a second time the direction of its curvature, the perpendicular corresponding to this second point of inflexion would necessarily be shorter than the perpendicular corresponding to the first, since it would have less obliquity, and would proceed from a point nearer the axis. But this is incompatible with the equation of equilibrium; for this equation being reduced at all the points of inflexion to $\frac{1}{N}=C$, the two above perpendiculars must be equal. The existence of this second point of inflexion being thus impossible, we see that beyond the first, the curve, which cannot (§ 2) attain the axis of revolution, must necessarily either tend towards an asymptote parallel to that axis, or else present at a finite distance a point where the tangent shall be parallel to the same axis.

That the first of these two conditions must be rejected is at once obvious; for at the extreme point where the curve would touch the asymptote the radius of curvature would be infinite, which would again reduce the equation of equilibrium at that point to $\frac{1}{N}=C$, and the perpendicular would there also be evidently shorter than at the point of inflexion. In the second case, the point where the tangent would become parallel to the axis of revolution cannot, on account of the evident inequality of the perpendiculars, be a second point of inflexion. It would then constitute a minimum of distance to the axis, and consequently a small arc extending on both sides of this minimum would generate a constriction which might be realised between two equal rings or disks. Now we have discussed all the possible partial figures of that nature. We have seen that every constricted portion pertains either to an unduloid or a catenoid, or to the part of a nodoid, which encompasses the summit of a nodus; but we

know that the convex partial figure with which we started is not portion of an unduloid, since its convexity exceeds the sphere; it is perceptible that it is not portion of a catenoid, and from what precedes we see that the above contraction cannot be portion of a node.

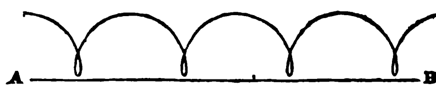
Thus our original hypothesis of a point of inflexion in the part of the curve which is withdrawing from the axis of symmetry and approaching the axis of revolution leads inevitably to impossibilities, and, consequently, the curve maintains the same direction of curvature until it deviates from those conditions. But to do so it is evidently necessary that it should first cease to withdraw from the axis of symmetry, or, in other terms, that it should present a point where the tangent is parallel to that axis. Neither is this point one of inflexion, for the perpendicular and the radius of curvature would there be both infinite, which would annul the quantity $\frac{1}{M}+\frac{1}{N}$. Beyond this point, then, the curve redescends towards the axis of symmetry, still preserving the direction of its curvature. Further, this same direction is maintained, as we shall show, so long as the curve continues to descend. In effect the liquid of the partial figure realized, and which has served us for a point of departure, being situated in the concavity of the curve, we readily see that at all the points of our descending branch the perpendicular is negative. But if this branch contained a point of inflexion the quantity $\frac{1}{M}+\frac{1}{N}$ would be reduced at that point to the term $\frac{1}{N}$, and consequently, on account of the sign of the perpendicular, would be also negative; while on the meridian arc of the realized partial figure the radius of curvature and the perpendicular being both positive, the quantity $\frac{1}{M}+\frac{1}{N}$ is itself positive.

But the branch in question cannot descend indefinitely by still approaching the axis of revolution, or, in other terms, cannot tend toward an asymptote parallel to that axis; for, at the point situated at infinity on the asymptote, the quantity $\frac{1}{M}+\frac{1}{N}$ would again be reduced to the term $\frac{1}{N}$, and consequently would be again negative; it is necessary, therefore, that one branch should pass at a minimum of distance from the axis of revolution, and should thus form the generating arc of a constriction; and as this constricted portion could pertain neither to the unduloid nor the catenoid, it necessarily constitutes the summit of a node of the nodoid. We must recur, therefore, to the meridian line of the nodoid, and conclude that all the figures obtained in the experiments of § 31 are partial nodoids, whatever the degree of approximation of the disks, provided the spherical curvature be overpassed, and whatever the volume of oil in relation to the diameter of the disks.

§ 34. We are now in a position to consider what is the nature and what the limits of the variations of the nodoid. Since, in the experiments of § 31, we pass by a portion of a sphere, after which, as has been just seen, the partial nodoid is immediately realized, and since the latter then varies continually until it reaches the phase at which instability commences, it is obvious that the portion of a sphere constitutes one of the limits of these variations, and that hence the limit of the corresponding variations of the complete nodoid is an indefinite series of equal spheres, having their centres on the axis. But it will readily be perceived that the only possible mode of continuous variation tending towards that limit is the following: in proportion as the complete nodoid approaches the series of spheres, the dimensions of the nodes as well as the distance of their summits from the axis diminish more and more, while the curvature of the arcs which connect these nodes verges towards that of the cir-

cumference of a circle having its centre on this same axis; finally, at the limit, the nodes entirely disappear, and the above arcs become so many demi-circumferences, tangents one to the other. The spheres, therefore, generated by these semi-circumferences are tangents also, and hence it results that one of the limits of the variations of the nodoid is an indefinite series of equal spheres, which touch each other upon the axis. We already know (§ 8) that a similar series of spheres constitutes one of the limits of the variations of the unduloid, so that this limit is common to the two figures, and forms consequently the transition from one to the other; this is likewise shown by the experiments of § 31, since, in passing from the cylinder to the portion of a sphere, the figure realized always pertains to the unduloid. The meridian line of a nodoid, but little remote from the limit just ascertained, is represented by Fig. 39.

Fig. 39



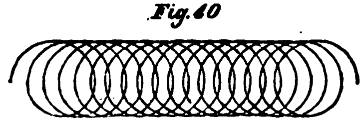
§ 35. The variations of the nodoid have a second and very remarkable limit. Let us realize, by the process explained in § 27, the portion of a nodoid generated by an isolated node; let us suppose, moreover, that we successively repeat the experiment with solid rings of constantly increasing size, and that we so modify the volume of oil that the length of the meridian node, that is, the distance from its summit to its point, shall remain the same. When the diameter of the solid ring is very considerable, the perpendiculars corresponding to the different points of the node will be all very large, so that at all these points the term $\frac{1}{N}$ of the equation of equilibrium will be very small, and we perceive that this term will tend towards zero in proportion as the diameter of the solid ring tends towards infinity; but it cannot be thus with the term $\frac{1}{M}$, for if this last also tended towards zero, the liquid figure would have for a limit of its variations the catenoid, which is evidently impossible under the conditions implied—that is to say, when the node is of constant length; we can always, then, imagine the solid ring so large that at all points of the meridian node the term $\frac{1}{N}$ shall be very small relatively to the term $\frac{1}{M}$. The latter, which expresses the meridian curvature, should now, in virtue of the equation of equilibrium, vary very little on the whole contour of the node, and consequently this will closely approximate to the circumference of a circle. It is clear that, in this case, the curvature of the arcs which connect the consecutive nodes of the complete meridian line will also be very nearly constant, and of the same order with that of the nodes, for the term $\frac{1}{N}$ will be also very small on the arcs in question. From this we perceive that the consecutive nodes of the meridian line will encroach upon one another, and that hence for a certain large diameter of the solid ring this line will have the form partially represented at Fig. 40. In this figure the axis of revolution is not indicated, because it is situated at too great a distance.

If we imagine the diameter of the ring still further enlarged, the meridian curvature will still more nearly approach uniformity; the nodes will be more nearly circular and will more closely encroach on one another; finally, at the

limit of such increase, when the diameter becomes infinite, the term $\frac{1}{N}$ will completely disappear for all points of the meridian line; which, as regards this entire line, will reduce the equation of equilibrium to $\frac{1}{M} = C$; the radius of curvature will be then strictly constant, and we shall arrive at this singular result, that the total meridian line will be condensed into a single circumference of a circle; and as the latter will be situated at an infinite distance from the axis of revolution, we perceive that the figure generated will be simply a cylinder. Thus the second limit of the variations of the nodoid is the cylinder; but this cylinder is placed transversely in relation to the axis of the nodoid from which it is derived, and that axis is infinitely removed from it; while the cylinder which forms the second limit of the variations of the unduloid (§ 8) has for its axis that of the latter figure.

§ 36. For the partial realization of a nodoid whose complete meridian line shall be of the kind represented by Fig. 40, it is not necessary that the absolute

Fig. 40



diameter of the solid ring should be very considerable; it is sufficient that this diameter be large relatively to the length of the meridian node. For, if we reflect that, in this latter, the curvature continues to diminish (§ 26) from the summit to the points where the tangents are parallel to the axis of symmetry, and that, from thence to the other extremity of the node, it is less than at those points, we shall perceive that if the length of this same node is small in relation to the radius of the solid ring, its width will be still smaller, and that at its summit the radius of curvature will be extremely small in comparison with the distance from that summit to the centre of the ring, a distance which constitutes the perpendicular. At the summit, then, the term $\frac{1}{N}$ will be inconsiderable in regard to the term $\frac{1}{M}$, and the value of the quantity $\frac{1}{M}+\frac{1}{N}$ will depend chiefly on that of $\frac{1}{M}$; but it is at the summit that the perpendicular is least; therefore, upon the rest of the node and upon the arcs which unite this node with the nodes neighboring on the complete meridian line, the term $\frac{1}{N}$ will have still less influence, and consequently, in the whole extent of that line, the curvature will vary but slightly.

The liquid ring momentarily obtained in a solid ring 70 millimetres in diameter, by piercing a disk reduced almost to a film, (2d series, § 19,) constitutes a partial nodoid of the kind which we are considering; this liquid ring has, in effect, but little size relatively to the radius of the solid ring. It is also evidently a concave portion of a nodoid of this kind which we realize in the experiments of § 25, when the disks are very near one another, and we stop the exhaustion of oil at the point where the extreme elements of the meridian arc are sloped on the faces of the disks at their borders. Such, too, is the figure realized in the experiments of § 31, when the distance of the disks is very small and the extreme elements of the meridian arc are inclined as nearly as possible on the prolongations of the solid faces. Here, however, the meridian arc does not appertain to

one single node: it is formed, as will be seen by Fig. 40, of the arc which unites two consecutive nodes and of two portions of the latter.

§ 37. The variations of the nodoid, finally, have, like those of the unduloid, a third limit, which is disclosed by the same experiments that have led us to a knowledge of the nodoid itself. In the experiments of §§ 20 and 22, when, after having formed a cylinder between two rings placed at a less distance than $\frac{2}{3}$ of their diameter, we progressively remove some of the liquid, the partial figure, as we have seen, becomes first an unduloid, then by degrees attains the catenoid, after which it immediately passes into the nodoid; whence it evidently follows that the catenoid is one of the limits of the variations of the nodoid, and, moreover, that it constitutes a new transition from the latter to the unduloid. We have already recognized (§ 34) another, consisting in an indefinite succession of spheres.

The third limit, then, of the variations of the nodoid is the catenoid, and it is easily made apparent how the figure becomes thus modified. If we consider that the experiments just spoken of realize the portion of the nodoid generated by an arc pertaining to a node, and presenting its concavity externally, we shall thence conclude that the portion of the nodoid which passes into the catenoid is that which is generated by one of the nodes, whose summit becomes that of the meridian catena. This being premised, let us conceive that each of the nodes of the complete meridian line becomes gradually modified to arrive at the catena, and let us imagine, for the sake of distinctness, that, during all these modifications, the distance of the summits from the axis of revolution remains constant. In proportion as the nodes approach the catena the quantity $\frac{1}{M}+\frac{1}{N}$ tends towards zero, but on all the arcs which unite the nodes with one another the quantities M and N are of the same sign, and consequently the quantity $\frac{1}{M}+\frac{1}{N}$ in relation to these arcs cannot tend towards zero unless M and N tend at the same time towards infinity; all the points, then, of these arcs will withdraw indefinitely from the axis of revolution, while their curvature becomes at the same time indefinitely weaker; whence it follows that the extremities of the nodes will withdraw further and further from the axis, while, by the increasing development of the intermediate arcs, which, from the nature of the curve, evidently cannot diminish in curvature without acquiring greater extension, the nodes will separate more and more from one another, until, at the limit, they are all infinitely distant and infinitely elongated. If, then, we consider one in particular, the whole curve will be reduced to that one alone, and, on the other hand, its extremity or point will have disappeared, and it will be found to be transformed into the meridian line of a catenoid, that is to say into a catena.

§ 38. A last question now presents itself: Are there other figures of equilibrium of revolution besides those of which we have thus far recognized the existence? All these last are such that portions of them can always be comprised between two equal and parallel disks. Now our experiments have exhausted all the combinations of that kind; whence we must conclude that if there were still other figures, they would be of such a nature as not to be capable of fulfilling that condition, and, for that, it would evidently be necessary that their meridian lines should present no point whose distance from the axis of revolution would be a maximum or a minimum. As these lines, moreover, could not reach the axis, (§ 2,) they must continue always to leave it, from a first point situated at infinity on an asymptote parallel to that axis, up to another point situated likewise at infinity. This being so, at the first of these two extreme points, the radius of curvature would be necessarily infinite, while the perpendicular would be finite, and the equation of equilibrium would be reduced to $\frac{1}{N}=C$; but from

this it results that the curvature could nowhere change its direction: for if there were a point of inflexion, the equation of equilibrium would be there also reduced to $\frac{1}{N}=C$, and consequently the perpendiculars at the above first extreme point, and at the point of inflexion, would be equal, which is evidently impossible. Therefore, the curve being free from all undulation, the curvature would necessarily tend towards zero, or, what amounts to the same, the radius of curvature would tend towards infinity in approaching the second extreme point, so that at that point the term $\frac{1}{M}$ would disappear as at the former, which would require, as before, the impossible equality of the two perpendiculars.

The sole figures, therefore, of equilibrium of revolution of a liquid mass withdrawn from the action of gravity are those at which we have arrived in the second and in the present series, namely: the sphere, the plane, the cylinder, the unduloid, the catenoid, and the nodoid. All these figures, with the exception of the sphere, having infinite dimensions in certain directions, it results that, among the figures of equilibrium of revolution, it is only the sphere which can be realized in a complete state with a finite mass of liquid; hence, as we have seen, it is always the spherical form which is assumed by a mass of oil abandoned to itself in our alcoholic mixture.

[TO BE CONTINUED IN THE NEXT REPORT.]

ARTIFICIAL SHELL-DEPOSITS IN NEW JERSEY.

BY CHARLES RAU, OF NEW YORK.

It has frequently been observed that there exists a certain resemblance between archæology and geology, notwithstanding the different character of the results obtained by these sciences, and the parallelism which they exhibit is really of sufficient distinctness to justify a comparison. By examining the petrified remains of animals and plants that are found in the layers composing the crust of the earth the geologist determines the different phases in the history of our planet; while the student of archæology, in endeavoring to throw light on the former condition of mankind, has to rely in a great measure on the ruins of buildings, on earthworks, implements of various kinds, organic remains, and other traces left by those who passed away long ago from the scene of life. But even in the results of the two sciences the analogy is not entirely wanting, in so far as the geologist, though succeeding in establishing the relative age of the strata, is unable to determine with any degree of certainty the time that was required to form the stony shells surrounding our globe; and in treating of ante-historic periods, the archæologist, likewise, is at a loss to fix the period when a people existed, of whose conditions of life, manners, and domestic habits he can give the most satisfactory account. I will mention in this place only two recent discoveries in archæology, namely, the *lacustrine villages* of Switzerland, Italy, and Germany, and the *Kjoekkenmoeddings* or *refuse-heaps* occurring on the Danish islands. In both cases we obtain, by the minute researches and ingenious conclusions of scientific investigators, a knowledge of certain populations concerning whom history is entirely silent; and while we have become acquainted with their character and manner of living, we neither know their names, nor are we able to determine the period when they inhabited those places which abound with tokens of their former existence. The lake-dwellings as well as the *Kjoekkenmoeddings* have been described in the Smithsonian publications* and elsewhere, and it would be useless to enlarge here on these subjects; but as I intend in this sketch to treat of American remains similar to the *Kjoekkenmoeddings*, I will merely devote a few words to the latter memorials of antiquity. On the coasts of the Danish islands and along the fjords of Jütland there occur extensive heaps of shells, mostly of the oyster, which were considered for a long time as formations of the sea, until of late their artificial character was established by Danish savans, who proved them to be the accumulated refuse of the repasts of a people that dwelt in former ages, beyond the record of history, on the shores of these islands.

The indications of the artificial origin of these shell-heaps chiefly consist in a total absence of stratification which always characterizes marine deposits, and in the fact that the rubbish contains rude flint implements, fragments of coarse pottery, fireplaces, charcoal, cinders, and the bones of various animals, some of which are now extinct in those parts, as for instance the urus, (*Bos urus* or *primigenius*,) beaver, and auk or penguin, (*Alca impennis, Lin.*) But neither bronze nor iron has been discovered in these places, from which it may be inferred that the inhabitants were unacquainted with the use of metals, and

*Annual Smithsonian Reports for 1860 and 1861.

belonged to that remote period which is called "the age of stone" by the archæologists of Europe.

From the islands of the Baltic sea I will now turn to the shores of New Jersey.

While spending, during the summers of 1863 and 1864, some weeks at Keyport, Monmouth county, New Jersey, a small town situated on Raritan bay, I examined within the precincts and in the neighborhood of that place several shell-deposits which are unmistakably artificial and the memorials of the Indians who formerly inhabited this region.* These deposits evidently owe their origin to the same causes which produced the Danish *Kjoekkenmoeddings*, to which they correspond in all essential points, constituting accumulations of cast-away shells, which sometimes merely form a more or less dense covering of the sandy surface, but also in a few instances beds or layers intermingled with sand and pebbles, in which case they assume the shape of irregular hillocks or mounds.

The shell-deposits of Keyport indicate the places where the aborigines were accustomed to feast upon the spoils of the neighboring beach, remarkable for its abundance of oysters, clams, and other eatable mollusks. They selected for this purpose favorably situated localities at some distance from the shore, and sufficiently elevated to be out of reach of high tide; and in a few cases that fell under my notice, the shell-beds are contiguous to creeks which run into the beach and probably afforded the means of transporting the supply of shell-fish in canoes from the sea directly to the place of encampment. The principal food of the aboriginal coast-population was evidently furnished by the common oyster (*Ostrea borealis, De Kay*) and the hard-shell clam (*Venus mercenaria, Lin.*) for their valves, partly very old and frequently broken, constitute almost entirely these accumulations of shells; but the common periwinkle (*Pyrula canaliculata* and *P. carica, De Kay*) is also often met, and was probably eaten by the aborigines, as it is at present by some of their Caucasian successors. I found only two or three specimens of the soft-shell clam (*Mya arenaria, Lin.*) among the shell-heaps, and none of the common black mussel (*Mytilus edulis, Lin.*) The last-named species, however, does not occur in great numbers in the neighborhood of Keyport, and the soft-shell clam has, as its name indicates, very thin and perishable valves, the fragments of which may lie buried among the thicker and more durable shells of the other mollusks. It would be rash, therefore, to suppose the soft-shell clam had been excluded from the bill of fare of the Indians. Among these remains of mollusks the broken bones of animals are occasionally met with, though generally in such an advanced state of decay that their character can no longer be determined; for, owing to the non-conservative quality of the sand which surrounds them, they have become entirely destitute of animal matter, and will almost crumble to pieces when handled for examination. The direct evidences of the occupancy of these places by the Indians are not wanting, and consist of numerous fragments of pottery and stone implements of the usual kind, otherwise very scarce in this part of New Jersey.

By far the most extensive shell-bed I had an opportunity to examine occurs on the farm of Mr. George Poole, situated a mile and a half northeast of Keyport, and about three quarters of a mile south of a small projection of the coast known as Conaskonck Point. The road leading from Keyport to the village of Union passes through the farm lands, which occupy an area of ninety acres. This locality was doubtless for many generations the abiding place, or at least the periodical resort, of the Indians, and traces of their former presence in the

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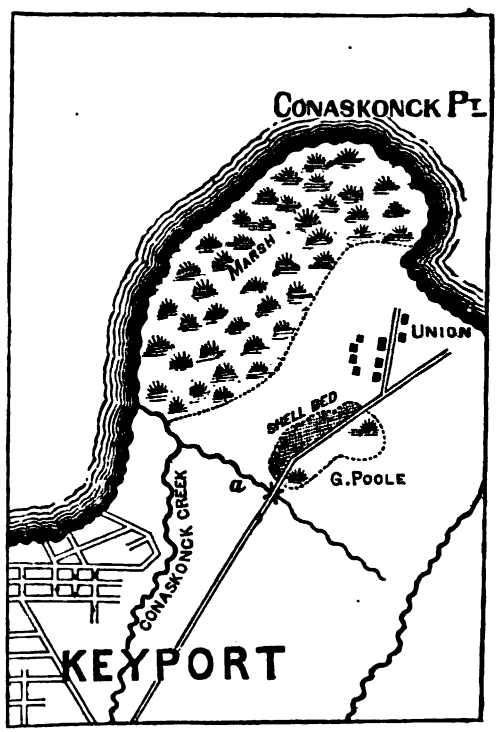
* My attention was first directed to these aboriginal remains by the Rev. Samuel Lockwood, a scientific gentleman of Keyport, who had recognized their true character before I made any investigations.

shape of cast-away shells, arrow-points, and broken pottery, may be discovered almost in every field belonging to the farm. Their principal camping-ground, however, was situated close to the road already mentioned, and is indicated by the dark dotted space on the accompanying plan. Here we have a *Kjoekkenmoedding* in the real sense of the word. Seen from a distance, this place has almost the appearance of a snow-covered field, owing to the great number of bleached shells constituting this deposit, which spreads over an area of six or seven acres and forms several extensive heaps or mounds of an average height of about five feet. But these heaps do not exclusively consist of shells: the latter are mostly imbedded in sand, probably carried thither by the action of winds—by æolic action, as science calls it—and intermingled with innumerable pebbles representing various mineral substances, among which those of the quartz family seem to predominate. As in other localities of the neighborhood, the shells on this spot are the remains of oysters, hard-shell clams, and periwinkles, the last-named kind of shell-fish being represented, as elsewhere, by a comparatively small number of specimens.



That considerable time was required to heap up these shells is evident, and, moreover, indicated by the chalky, porous appearance and fragility of many of the valves, while those that were cast away at later periods exhibit these signs of decay in a far less degree, and are even sometimes as sound as though they had but lately been left on the shore by high water. A great number of the shells are broken, especially those of clams, which seem to be more brittle than oyster shells. This breaking into fragments is caused by the sudden changes of temperature, in consequence of which the valves crack and ultimately fall to pieces. Concerning the depth of this deposit, I learned that about twelve years ago several hundred loads of shells were taken away from a certain spot for making a road. The excavation thus produced reached about eight feet downward, and the mass was found to consist throughout that depth of shells, sand, and pebbles. My own diggings, which were, however, of a more superficial character, led to the same result. This shell-bed is about half a mile distant from the shore at low tide, and the intervening area consists chiefly of so-called salt-meadow. In transporting the shell-fish to the camping place it is probable that the aborigines availed themselves of a small nameless creek (marked *a* on the plan) running towards the sea, west of the shell-bed, and not very distant from it. This creek, though rather narrow, is sufficiently deep for canoe navigation during high water, and joins the more considerable Conaskonck creek, which flows into the beach. There was, consequently, a water connexion between the sea and the camp. The space enclosed by a dotted line on the accompanying plan indicates the continuation, or rather the running out, of the shell-bed just described; for here the shells

are by far less numerous, and form no longer heaps, but lie thinly scattered over the ground, which is partly under cultivation, and swampy in some places, as marked in the drawing, by which it is only intended to show approximately the location and extent of the deposit.

By searching among these shell-heaps and in the adjacent fields I obtained more than three hundred specimens of Indian manufacture, consisting of stone axes, arrow and spear-points of different shapes, flint knives, and many pieces of broken crockery. The tomahawks, which consist of greenstone or sandstone, are of the usual shape, and encircled with a groove for attaching them to a handle. The material of the arrow and spear-heads is either flint, common quartz, greenstone, or a kind of dark slate. The specimens made of the two last-named mineral substances have a rather clumsy appearance, owing to the roughness of the material; but those wrought of flint are mostly well shaped and present pretty good samples of aboriginal art. That the manufacture of arrow-heads was carried on in this place is evident from the great number of flint chips which lie scattered among the shells; and, moreover, I picked up several unfinished arrows, which were thrown aside as useless in consequence of a flaw or wrong crack, or some other irregularity in the material. These specimens are in so far interesting as they illustrate the process of arrow-making. The fragments of pottery which I collected here consist of a dark clay, either mixed with coarse sand, or pure, and for the most part rather slightly burnt; some of the sherds still bear the ornamental lines and notches cut in the surface of the vessels. The mixing of the clay with pounded shells does not seem to have been practiced by the Indians of this region. I found also a fragment of an apparently large vessel cut out of a talcose stone. A few clay beads were picked up on the spot, but I did not obtain any of them.

The last Indians who visited periodically the neighborhood of Keyport, even within the recollection of old people, belonged, according to the statement of my informant, to the tribe of Narragansetts. They made their appearance every year and caught shell-fish, which they dried for winter use. Their encampment, however, was not on the spot of which I have given a description, but in Pleasant Valley, a little less than four miles south of Keyport.

I am informed that similar shell-beds occur on Long Island, where the neighboring farmers use the shells for burning lime. Two centuries and a quarter ago the Dutch colonists of Manhattan island made the same use of the shells heaped up by the Indians of that locality. The account of New Netherland given by the Jesuit missionary Isaac Jogues, contains the following passage relative to the subject:

"There are some houses built of stone; lime they make of oyster shells, great heaps of which are found here, made formerly by the savages, who subsist in part by that fishery." *

Sir Charles Lyell saw on St. Simon's island, near the mouth of the Altamaha river, in Georgia, large Indian shell-mounds, of which he gives the following description:

"We landed on the northeast end of St. Simon's island, at Cannon's Point, where we were gratified by the sight of a curious monument of the Indians, the largest mound of shells left by the aborigines in any one of the sea islands. Here are no less than ten acres of ground, elevated in some places ten feet, and on an average over the whole area five feet, above the general level, composed throughout that depth of myriads of cast oyster shells, with some mussels, and here and there a modiola and helix. They who have seen the Monte Testaceo,

* Memoir of a Captivity among the Mohawk Indians, a Description of New Netherland in 1642-'43, and other Papers, by Father Isaac Jogues, of the Society of Jesus, with a Memoir of the Author, by John Gilmary Shea, (New York, 1857,) p. 57. In the original the passage runs thus: "Il y a quelques logis bastys de pierre; ils font la chaux avec des coquilles d'huistres dont il y a de grans monceaux faits autrefois p les sauvages, qui vivent en partie de cette pesche."

near Rome, know what great results may proceed from insignificant causes where the cumulative power of time has been at work, so that a hill may be formed out of the broken pottery rejected by the population of a large city. To them it will appear unnecessary to infer, as some antiquaries have done, from the magnitude of these Indian mounds, that they must have been thrown up by the sea. In refutation of such an hypothesis, we have the fact that flint arrow-heads, stone axes, and fragments of Indian pottery have been detected throughout the mass." *

The same author noticed shell-deposits on the coasts of Massachusetts.

During his voyage round the world Mr. Darwin saw shell-heaps in the island of Terra del Fuego. He says:

"The inhabitants, living chiefly upon shell-fish, are obliged constantly to change their place of residence; but they return at intervals to the same spots, as is evident from the piles of old shells, which must often amount to many tons in weight. These heaps can be distinguished at a long distance by the bright green color of certain plants which invariably grow on them." †

We may expect to meet with artificial shell-accumulations, or at least traces of them, almost in all parts of the American coasts where an aboriginal population existed, and they have already been found in various places besides those mentioned, as for instance in Newfoundland and in California, and we shall doubtless hear of further discoveries as soon as proper attention is paid to these memorials of the native inhabitants of the American continent.

The occurrence of the Danish refuse-heaps, whose age is lost in the dawn of history, and of similar comparatively recent deposits in America, shows that the conditions of existence of those Baltic islanders and the American coast inhabitants were essentially the same, and furnishes a striking illustration of the similarity in the development of man in both hemispheres. A thorough investigation of the American shell-mounds will not only enable us to compare them more minutely with the corresponding remains of Europe, but may, possibly, disclose important facts relative to the former condition of the American race, and thus enlarge our stock of ethnological knowledge.

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* A Second Visit to the United States of America, by Sir Charles Lyell, (New York, 1849,) vol. i, p. 252.

† Journal of Researches, &c., by Charles Darwin, (New York, 1846,) vol. i, p. 272.

THE INTERMIXTURE OF RACES.

BY GEORGE GIBBS.

The subject of the intermixture of races, and its result as affecting the physical development of the ensuing progeny, is one of the most interesting in anthropology, especially in its bearing upon the question of the unity of the human family. Yet, so far as this continent is concerned, it has nowhere received the thorough, systematic, and conscientious investigation which it deserves. What observations have been made, are, so far as I have seen, confined to the union of whites and negroes. Even as to Mexico, where the mixed races form so large a part of the population, the inquiry seems to have been generally neglected.

During a residence in Oregon, commencing in 1849, before the great influx of American emigrants, and when the proportion of half-breeds to the fur-traders and other early settlers was easily perceivable, my attention was drawn to the fact that, notwithstanding the long intercourse of these with native women, their offspring formed but a small element in the community. Being anxious to ascertain whether this was due to a taint common among the coast tribes and other causes of merely local influence, or whether it was of general extension through the northern and temperate parts of America, I subsequently addressed a letter to the Right Reverend Bishop Taché, of the Red River Settlement, the substance of which, and his reply, is given below. In the case of the white and black races, the weight of testimony is certainly unfavorable to the health and longevity of the offspring, and the impression has been general that this was also the case with Indian half-breeds, at least in the northern temperate climates. Any trustworthy observations on this question are, therefore, important, and the testimony of that gentleman is beyond cavil. It is hoped that this communication may lead to investigation among the civilized tribes of the west, the Cherokees, Choctaws, and Creeks.

I should premise that syphilis has long prevailed in the coast region of Oregon, that is to say, in the country west of the Cascade range, it having been noticed by Lewis and Clarke as early as 1806. The erotic temperament of these tribes, common to all people whose food is chiefly fish, coupled with the absence of moral restraint, has tended to disseminate it widely, and thus been one cause of the dying out of the aborigines. Its effects, owing to climate, food, and general mode of life, are less fatal, it is true, in the first instance than among some other nations, but they show themselves in the prevalence of scrofulous diseases, in the diminished number of children, and in their early death.

This state of facts, however, applies less to the case of the half-breeds than to the unmixed Indians, for the reason that the selections of the whites were usually from the better class of females, and to a considerable extent from the interior tribes, where disease is far less common. The fathers themselves were of three races, Scotch, Canadian French, and Americans, all hardy and vigorous men; their families were, of course, better fed and cared for than those of the savages; the climate is proverbially healthy, and yet this mixed population has not increased as might have been expected.

SMITHSONIAN INSTITUTION, *Washington, D. C., June* 17, 1862.

REVEREND AND DEAR SIR: Being engaged in the preparation of a work on the Indians of Northwestern America, among whom I have resided a number of years, I am desirous of comparing my observations on some points of vital statistics with those of other parts of the country. Among these the question of intermarriage of the two races is prominent, and I take the liberty of applying to you for information upon it.

As a general thing the metifs of Oregon have been short-lived, and it is at once noticeable that in the length of time which has elapsed since the entrance of the fur-traders into that country, (a half century,) and the great number of marriages that have taken place with native women, only a very small indigenous mixed population has sprung up. Yet at the same time the half-breeds who arrive there from the Red River country appear healthy, and the men strong and able-bodied. The cause of mortality does not arise from vice only, for it is noticeable in the families of the better class, as well as among the lower. As regards the intermarriage of Oregon half-breeds among themselves, I do not know a single case where they have left offspring. You, on the other hand, have a large mixed population, and they must, of course, intermarry. They have the reputation of being a hardy, athletic, and vigorous people, and I am curious to know in what the difference, if any there is, consists.

Will you, therefore, be kind enough to inform me, as nearly as possible, as follows:

1. The actual number of the mixed race in the Red River colony.
2. The average duration of life by estimation, if not otherwise attainable absolutely, and as compared with that of white settlers.
3. Whether instances of prolonged life are frequent.
4. Whether there seems a marked difference in longevity between men and women.
5. Whether marriages between metifs are common; and if so, whether they are as prolific as those between white persons, or between Indians; and whether the offspring of such intermarriages are as vigorous and long-lived as the results of the first cross of the two races.
6. Whether this class of population is increasing, and likely to result in a permanent mixed race or variety of the human species. This question is the more interesting, as I suppose your pure white settlers to be a fair-haired race, which has in general not crossed as well with the Indians as the darker nations, such as the Spanish and Portuguese, and because mixed races seem always to have thriven better in warm than in temperate or cold climates.

I am, reverend and dear sir, very respectfully,

GEORGE GIBBS.

Right Rev. Bishop TACHÉ.

DIOCESE OF ST. BONIFACE, RED RIVER SETTLEMENT,
Hudson's Bay Territory, July 21, 1862.

DEAR SIR: I have the honor to acknowledge the receipt of your interesting favor of the 17th ultimo, which duly came to hand by the last mail. You certainly have no need of apology for having addressed me on the points mentioned in your letter. I only regret my inability to satisfy you as fully as I might wish. The burning of my cathedral and palace, with all the archives of the bishopric, renders it impossible for me to be very precise. The little information in my possession on the subject I will cheerfully give, trusting that it may be of help to you in your scientific labors.

I now proceed to answer your questions. The answer to your first query will be found in the annexed copy of a statistical table from the official census of this settlement taken in 1856.

2d. We have as many instances of longevity among the half-breeds as among the white population.

3d. Having lost my register, I cannot ascertain the average duration of life here, but I consider it as about equal to that of the white settlers of this country, and far above that of the unmixed Indians.

4th. I remark no difference in longevity between the sexes.

5th. We have daily instances of marriages between half-breeds. They generally have numerous children, who are as long-lived and vigorous as the first crosses of the two races.

6th. This class of population is rapidly increasing, and is sure to result in a permanent mixed race or variety of the human species, and it is not kept up chiefly by additions from without.

7th. Fair-haired white settlers have crossed as well with the Indians as those of dark complexion. No mixed race can ever have thriven better in warm climates than in this extremely cold one.

You will easily comprehend by the above answers my utter surprise on seeing your statement about the Oregon half-breeds.

I beg leave to remain, in conclusion, your obedient servant,

† ALEX. TACHÉ,
R. C. Bishop of St. Boniface, O. M. I.

Census of the Red River colony, taken May 20, 1856.

Year.	Total number of families.	RELIGION.		NATIVE COUNTRY.							POPULATION.								Total.
											Men.		Women.		Boys.		Girls.		
		Protestants.	Catholics.	England.	Ireland.	Scotland.	Canada.	Norway.	Rupert's Land half-breeds.	Switzerland.	Married.	Unmarried.	Married.	Unmarried.	Above 16 years.	Under 16 years.	Above 15 years.	Under 15 years.	
1856..	1,095	553	542	40	13	119	92	1	828	2	999	230	1,010	285	536	1,486	562	1,583	6,691
1849..	1,052	....	....	....	....	....	....	....	....	....	873	145	877	135	382	1,314	373	1,292	5,391
Increase in 7 years				....	....	....	....	....	....	....	....	....	....	....	....	....	....	....	1,300

AN ACCOUNT

OF

THE ABORIGINAL INHABITANTS

OF

THE CALIFORNIAN PENINSULA,

AS GIVEN BY

JACOB BAEGERT, A GERMAN JESUIT MISSIONARY, WHO LIVED THERE SEVENTEEN YEARS DURING THE SECOND HALF OF THE LAST CENTURY.

TRANSLATED AND ARRANGED BY CHARLES RAU.

(Continued from the Smithsonian Report for 1863.)

CHAPTER V.—THEIR CHARACTER.

In describing the character of the Californians, I can only say that they are dull, awkward, rude, unclean, insolent, ungrateful, given to lying, thievish, lazy, great talkers, and almost like children in their reasoning and actions. They are a careless, improvident, unreflecting people, and possess no control over themselves, but follow, in every respect, their natural instincts almost like animals.

They are, nevertheless, like all other native Americans, human beings, real children of Adam, and have not grown out of the earth, or of stones, like moss and other plants, as a certain impudent, lying freethinker gives to understand. I, at least, never saw one growing in such a way, nor have I heard of any of them who originated in that peculiar manner. Like other people, they are possessed of reason and understanding, and their stupidity is not inborn with them, but the result of habit; and I am of opinion that, if their young sons were sent to European seminaries and colleges, and their girls to convents where young females are instructed, they would prove equal in all respects to Europeans in the acquirement of morals and of useful sciences and arts, as has been the case with many young natives of other American provinces. I have known some of them who learned several mechanical trades in a short time, often merely by observation; and, on the contrary, others who appeared to me duller, after twelve or more years, than at the time when I first became acquainted with them. God and nature have endowed these people with gifts and talents like others; but their rude life hinders the development of these faculties, and thus they remain awkward, dull, and so slow in their understandings that it requires considerable pains, time, and patience to teach them the doctrines and precepts of the Christian faith, insomuch that a sentence of only a few words must be repeated to them twelve times and oftener before they are capable of reciting it.

It may not be out of place to corroborate here what Father Charlevoix says of the Canadians, namely, that no one should think an Indian is convinced of what he has heard because he appears to approve of it. He will assent to anything, even though he has not understood its meaning or reflected upon his answer, and he so does either on account of his indolence or indifference, or from motives of selfishness, in order to please the missionary.

The Californians do not readily confess a crime unless detected in the act, because they hardly comprehend the force of evidence, and are not at all ashamed of lying. A certain missionary sent a native to one of his colleagues with some loaves of bread and a letter stating their number. The messenger ate a part of the bread, and his theft was consequently discovered; another time, when he had to deliver four loaves, he ate two of them, but hid the accompanying letter under a stone while he was thus engaged, believing that his conduct would not be revealed this time, as the letter had not seen him in the act of eating the loaves.

In the mission of St. Borgia the priest ordered his people one day to strew the way with some green herbs, because he was about to bring the holy sacrament to a sick person, and his order was promptly executed by them, but to the great damage of the missionary's kitchen-garden, for they tore up all the cabbages, salad, and whatever vegetables they found there, and threw them on the road.

Yet, notwithstanding their incapacity and slow comprehension, they are, nevertheless, cunning, and show, in many cases, a considerable degree of craftiness. They will sell their poultry to the missionary at the beginning of a sickness, and afterwards exhibit a disposition to eat nothing but chicken-meat, till none of the fowls are left in the coop. A prisoner will feign a dangerous malady and ask for the last sacrament in order to be relieved from his fetters, and to find, subsequently, a chance to escape. They rob the missionary in a hundred ways, and sometimes in the most artful manner. If, for instance, one has pilfered the pantry and left it open in his haste, another one forthwith requests to be admitted to confession, in order to give the thief time for closing the door, and thus to remove all cause of suspicion on the part of the missionary. They also invent stories and relate them to their priest for the purpose of frustrating a marriage engagement, that some other party may obtain the bride. These and many hundred similar tricks have actually been played by them, and show conclusively that they are well capable of reasoning when their self-interest or their needs demand it.

The Californians are audacious and at the same time faint-hearted and timid in a high degree. They climb to the top of the weak, trembling stems, sometimes thirty-six feet high, which are called *cardones* by the Spaniards, to look out for game, or mount an untamed horse, without bridle and saddle, and ride, during the night, upon roads which I was afraid to travel in the daytime. When new buildings are erected, they walk on the miserable, ill-constructed scaffoldings with the agility of cats, or venture several leagues into the open sea on a bundle of brushwood, or the thin stem of a palm-tree, without thinking of any danger. But the report of a gun makes them forget their bows and arrows, and half a dozen soldiers are capable of checking several hundred Californians.

Gratitude towards benefactors, respect for superiors, parents, and other relations, and politeness in intercourse with fellow-men, are almost unknown to them.* They speak plainly, and pay compliments to no one. If one of them has received a present, he immediately turns his back upon the donor and walks off without saying a word, unless the Spanish phrase, *Dios te lo pague*, or, "God reward you," has been previously, by a laborious process, enforced upon his memory.

Where there is no honor, shame is ever wanting, and therefore I always wondered how the word "*ié*," that is, "to be ashamed," had been introduced

* According to Baegert's own statement, (p. 309,) the forced departure of the Jesuit missionaries from the peninsula caused great distress among the Indians, who expressed their grief by a general howling and weeping, which shows that the feelings of gratitude and attachment were not entirely wanting in their character, although selfishness may have had a large share in the demonstration. The parting scene is well described in a few lines by W. Irving.—*Adv. of Captain Bonneville*, p. 332.

into their language; for, among themselves, no one would blush on account of any misdeed he had perpetrated. If one had killed his father and mother, robbed churches, or committed other infamous crimes, and had been a hundred times whipped and pilloried, he would, nevertheless, strut about with a serene brow and an erect head, and without being in the least degraded in the eyes of his people.

Laziness, lying, and stealing are their hereditary vices and principal moral defects. They are not a people upon whose word any reliance can be placed, but they will answer in one breath six times "yes" and as many times "no," without feeling ashamed, or even perceiving that they contradict themselves. They are averse to any labor not absolutely necessary to supply them with the means of satisfying hunger. If any work occurred in the mission, it was necessary to drive and urge them constantly to their task, and a great number complained of sickness during the week-days, for which reason I always called the Sunday a day of miracles, because all those who had been sick the whole week felt wonderfully well on that day. If they were only a little more industrious, they might improve their condition, to a certain extent, by planting some maize, pumpkins, and cotton, or by keeping small flocks of goats, sheep, or even a few cattle; and, having now learned to prepare the skins of deer, they could easily supply themselves with garments. But nothing of this kind is to be expected of them. They do not care to eat pigeons, unless they fly roasted into their mouths.* To work to-day and to earn the fruit of their labor only three or six months afterwards seems to be incompatible with their character, and for this reason there is little hope that they will ever adopt a different mode of life.

Books could be filled with accounts of their thefts. They will not touch gold or silver; but anything that can be chewed, be it raw or cooked, above the ground or below, ripe or unripe, is not more safe from them than the mouse from the cat, if the eye of the owner be only diverted for a moment. The herdsman will not even spare the dog that has been given to him to watch the flock of sheep or goats intrusted to his care. While one day observing, unseen, my cook, who was engaged in boiling meat, I noticed that he took one piece after another out of the kettle, bit off a part, and threw it again into the vessel. The meal on the missionary's table, when he is suddenly called away, is not safe from their thievery, and even the holy wafers in the sacristy are in danger of being taken by them. Yet they sometimes lay their hands on things of which they can make no use whatever, in a way really surprising, which shows to what degree stealing has become a habit with them.

For eight years I kept, ranging at large, from four to five hundred head of cattle, and sometimes as many goats and sheep, until the constant robberies of the Indians of my own and the neighboring mission compelled me to give up cattle-breeding.† In the bodies of nineteen cows and oxen, that had been killed in one day in the mission, there were found, after the removal of the skin, more than eight flint-points of arrows, the shafts of which had been broken off by the wounded animals while passing through the rocks and bushes. I believe that more of these animals were killed and eaten by the natives than were brought to the mission for consumption, and horses and asses suffered in like manner.

* German proverb.

† The cattle, as well as the goats and sheep, are described as small and lean, owing to the scanty pasturage. The horses, though small, were of a good breed and enduring, but they did not sufficiently multiply, and fresh animals had to be imported every year to mount the soldiers and cowherds. "The ass alone," says the author, "which is nowhere choice, but always contented, fares tolerably well in California. He works but little, and feeds on the prickly shrubs with as much relish as if they were the most savory oats." The number of hogs on the whole peninsula hardly amounted to a dozen.

In order to be exempt from labor, or to escape the punishment for gross misdeeds, the Californians sometimes counterfeit dangerously sick or dying persons. Many of those who were carried to the mission in such a feigned state by their comrades received a sound flogging, which suddenly restored them to health. Without mentioning all the cases that fell under my notice, I will speak of two individuals who represented dying persons so well that I did not hesitate to give them extreme unction. Another really frightened me by pretending to be infected with the smallpox, which actually raged in the neighboring mission, causing its priest for three months, day and night, a vast deal of trouble and care, and keeping him almost constantly on horseback. A fourth, whose name was Clement, seemed also resolved to give up the ghost. With him, however, the difficulty was that he had never seen a dying person, not even his wife, whom I had buried, and often visited during her sickness, without ever finding the husband at home. But having witnessed the death of many cows and oxen, which his arrows had brought down, he imitated the dying beast so naturally, by lolling out his tongue and licking his lips, that he went afterwards always by the name of *Clemente vacca* or *Cow Clement.*

Nothing excites the admiration of the Californians. They look upon the most splendid ecclesiastic garments, embroidered with gold and silver, with as much indifference as though the material consisted of wool and the galoons of common flax. They would rather see a piece of meat than the rarest manufactures of Milan and Lyons, and resemble, in that respect, a certain Canadian who had been in France, and remarked, after his return to Canada, that nothing in Paris had pleased him better than the butcher-shops.*

They are not in the least degree susceptible of disgust, but will touch and handle the uncleanest objects as though they were roses, killing spiders with their fists, and taking hold of toads without aversion. They use as a covering the filthiest rag, and wear it until it rots on their bodies. In person they are exceedingly dirty, and waste hardly any time in decorating and embellishing themselves. I must mention here, also, that they are in the habit of washing themselves with urine, which renders their persons very disagreeable, as I have often experienced when I had to confess them. I was informed by reliable people that they eat a certain kind of large spiders, and likewise the vermin which they take from each other's heads; but I never saw them doing it: whereas I saw them frequently fetch their maize porridge at noon in a half-cleaned turtle-shell which they had used the whole morning to carry the dung from the folds of the sheep and goats.

Concerning their improvement by the introduction of the Christian religion, I am unable to bestow much praise upon those among whom I lived seventeen years, during which period I had sufficient opportunity to become thoroughly acquainted with their character; but I must confess, to my greatest affliction, that the seed of the Divine Word has borne but little fruit among them; for this seed fell into hearts already obdurated in vice from their very infancy by seduction and bad example, which all pains and exertions on the part of the missionary were unavailing to remove. The occasions for evil-doing, among young and old, are of daily occurrence, and numberless. The parents themselves give the worst example, and the Spanish soldiers, cowherds, and a few others who come to the country for the purpose of pearl-fishing and mining, contribute not a little to increase vice among the native population. The mo-

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* Mr. Catlin relates a similar circumstance of a party of Iowa Indians that were exhibited in London. After their first drive through the city, "they returned to their lodgings in great glee, and amused us at least for an hour with their first impressions of London, the leading, striking feature of which, and the one that seemed to afford them the greatest satisfaction, was the quantity of fresh meat that they saw in every street hanging up at the doors and windows."—*Catlin's Notes of Eight Years' Travels and Residence in Europe.* New York, 1848: vol. ii, p. 9.

tives, on the other hand, which act elsewhere as checks upon the conduct of the people, and keep them within the bounds of decency, are not at all understood or appreciated by the Californians, for which reason the teachings of religion can make but little impression upon their unprepared minds; and being thus unrestrained by any considerations, they easily yield to the impulses of their character, in which a strong passion for illegal sexual intercourse forms a prominent feature. In all bad habits and vices the Californian women fully equal the men, but surpass them in impudence and want of devotion, contrary to the habit of the female sex in all the rest of the world. There were certainly some among the Californians who led edifying lives and behaved in a praiseworthy manner after having embraced the Christian faith; but their number was very small; the reverse, on the contrary, being the general rule to such a degree that the wicked and vicious formed the great majority of the natives.

CHAPTER VI.—THEIR CHARACTER, CONTINUED.—AN ACCOUNT OF THE ASSASSINATION OF THE JESUIT FATHERS TAMARAL AND CARRANCO.*

To all other bad qualities of the Californians may be added their vindictiveness and cruelty. They care very little for the life of man, and an insignificant cause will stimulate them to commit a murder. Among other cases which happened while I lived in their country, I will mention that of the master of a small ship loaded with provisions for two poor missions. This man had scolded a number of natives for some cause or other, which they resented by breaking his skull with a heavy stone, while he was eating his supper on the shore. His ship they abandoned to wind and waves. In the year 1760, a boy of about sixteen years stabbed another of the same age with a knife in the abdomen, and struck him on the head with a heavy club, almost within sight of the whole tribe, and only a stone's throw from the church and the house of the missionary. The murderer had already selected a horse on which to escape, and intended to save himself within a church thirty leagues distant from the place where the crime was committed; but he failed to effect his flight.†

Up to the year 1750 the Californians had revolted at different times and places, and compelled several missionaries to abandon their stations, and to seek safety in other quarters. The natives were stirred up to these insurrections either by their conjurers or sorcerers, whose influence had been considerably reduced, or because it was requested of them to keep those promises which they had made when receiving the holy baptism.

The most extensive and dangerous revolt of all began in the year 1733, in the southern part of the peninsula, among two tribes called the *Pericúes* and *Coras*, who are to this day of a very fierce, unruly, and untractable character, and who gave much trouble to Father Ignatius Tirs, from Kommotau, in Bohemia, the last Jesuit missionary who resided in their district.‡

In the year 1733 there existed in that part of the country, which was inhabited by several thousand natives, four missions, with three priests, who had in all only six soldiers for their protection. The missions were the following: *La Paz*, without a resident priest, and guarded by one soldier; *St. Rosa*, under Father Sigismund Taraval, a Spaniard, born in Italy, protected by three soldiers; *St. Yago*, over which Father Lorenzo Carranco, a Mexican, of Spanish

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* This episode in the missionary history of California forms a separate chapter in the third part of our author's work; but as it throws much light on the temperament of the natives, I have inserted it in this place.

† This church was probably considered as an asylum or place of safety.

‡ He was one of those who shared with the author, in 1767, the fate of banishment. At that time there were in all sixteen Jesuits in Lower California—fifteen priests and one lay brother. Six of them were Spaniards, two Mexicans, and eight Germans. The names of the latter are given on page 312 by the author, who omits, however, his own name in order to preserve his anonymous character.

parentage, resided, with two soldiers; and *St. Joseph del Cabo*, under Father Nicolas Tamaral, from Sevilla, in Spain, without any guard.

The motives leading to this insurrection, which were afterwards freely divulged by the natives, consisted in their unwillingness to content themselves with one wife, although they had promised to renounce polygamy, and their displeasure at being reprimanded for certain transgressions deserving the censure of their spiritual advisers. The ringleaders and principal movers of the rebellion were two individuals, *Botòn* and *Chicóri* by name, who exerted a great influence among the natives, and prepared everything in secret for the outbreak. Their object was to kill the three priests, to exterminate all traces of Christianity, which most of them had adopted ten years before, and to resume their former loose and independent manner of living. Their design became, however, known, and the fire was extinguished before it could blaze up in full flames. The Indians feigned a friendly disposition, and a kind of peace was established towards the beginning of the year 1734. But as this peace was not concluded with sincerity, it could not be of a long duration. The treacherous rebels soon again made attempts to carry out at all hazards the objects they had in view, and really succeeded in the following October, though not so completely as they wished, since Father Taraval found the means to escape their murderous hands.

The six soldiers were their principal obstacle. Meeting in the field with one of them of the mission of St. Rosa, they assassinated him, and sent word to the mission that he was very ill, requesting the priest either to come to the place in order to confess him, or to order the two remaining soldiers to transport the patient to the station, their intention being to decoy the one or the others, and to take their lives. But fortunately the messenger delivered his commission in such an awkward manner that the crime they had already perpetrated, as well as their further designs, could be easily divined, for which reason neither the priest nor the soldiers complied with their request. A few days afterward they killed also the only soldier belonging to the mission de la Paz.

The rumor of these two murders, and other indubitable signs of an impending mutiny and general uprising in the south, were spread abroad, and soon reached the ears of the Superior of the missions, who was then at that of the Seven Dolors, nearly ninety leagues from the place where these events had occurred. He sent orders immediately to the three priests whose lives were endangered to save themselves by flight, but the letters fell into the hands of the mutineers, and would, besides, at any rate have arrived too late to avert the peril.

It was the intention of the conspirators to strike the first blow against the mission of St. Joseph and Father Tamaral; but learning that Father Carranco had already received intelligence of their plans, they rushed with all speed upon his mission before he could make any preparations for defence, or effect his escape from the place. It was on a Saturday, and the 2d of October, when they arrived at the mission of St. Yago. The father had just said mass, and had locked himself in his room to perform his private devotions. Most unfortunately the two soldiers, who formed his whole body-guard, had left the place on horseback in order to bring in some head of cattle for the catechumens and other people of the mission. After a while the returned messengers, whom Father Carranco had despatched to the mission of St. Joseph to warn Father Tamaral of the danger to which he was exposed, entered the room. Father Carranco was reading his answer, when the murderers entered the house and fell upon him. Some threw him on the ground and dragged him by his feet to the front of the church, while others pierced his body with many arrows, and beat him with stones and clubs till he expired.

A little native boy, who used to wait upon the father when he took his meals, was a witness to the act, and shed tears when he beheld his benefactor's mournful fate; upon which one of the barbarians seized the boy by the legs and smashed his head against the wall, saying, that since he showed so much

regret at the death of his master, he should also serve him and bear him company in the other world. Among the murderers were some whom the father had considered as the most reliable of his flock, and whose fidelity he never had doubted.

Having torn the garments from the lifeless body, they treated it in a most abominable manner in order to wreak their vengeance, and they finally threw it on a burning pile. After this they set the church and the house on fire, and burned to ashes the utensils of the church, the altar, the representations of our Saviour and of the Saints, and everything else that they could not apply to their own use. In the mean time the two unarmed soldiers, who had been sent after cattle, returned. They were compelled to dismount and to kill the cows for the malefactors, after which the savages despatched them with a shower of arrows.

On the following day, the same fate befell Father Tamaral, the priest of the mission of St. Joseph, twelve leagues distant from that of St. Yago, for as soon as the villains had committed their crime at the one place, they directed their march to the other. Father Tamaral, not believing the report of his colleague, was quietly sitting in his house, when the savage crowd, considerably increased by members of his own parish, made their appearance in the mission. In their usual manner, they demanded something from the missionary, for the purpose of finding a pretext for quarrelling and commencing their hostilities, in case the priest should disappoint them in their wishes. But their behavior, and the arms which they all carried with them, soon convinced the missionary that they had other designs, and he consequently not only complied with their requests, but gave them even more than they demanded. Being thus baffled in their attempt, and full of eagerness to carry out their bloody plan, they put aside all dissimulation and attacked the missionary without further delay. They threw him on the ground, dragged him into the open air, and discharged their arrows upon him. One of their number, whom the father had a short time before presented with a large knife, added ingratitude to cruelty by burying the weapon in his body.

Thus the Fathers Tamaral and Carranco were led to the shambles by their own flock, and closed their days in California, after they had spent many years in that country, and, by a blameless life and great zeal, proved themselves worthy to die the death of martyrs. The abuses to which the savages subjected the body of the deceased priest were greater, in this instance, and they exhibited more wantonness in the destruction of the church and other property than on the preceding day, because the crowd was larger and had become more infuriated by previous success.

Father Taraval, of St. Rosa, the third priest of whom they intended to make a victim, succeeded in making good his flight. He sojourned for the moment on the western coast of California, at the station of All Saints, which formed an adjunct to his own mission, and was a two days' journey distant from St. Joseph. Being warned in due time by some faithful Indians of the danger that threatened him, he packed up in great haste his most needful things and rode at full speed, in company with his two soldiers, during the night of the fourth of October towards the opposite shore of the peninsula, where he embarked near the mission of La Paz in a small vessel, which had been despatched to that place when the first news of the impending rebellion became known. He landed in safety at the mission of the Seven Dolors, then situated near the sea; leaving behind him the smoking ruins of four missions that had been totally destroyed in less than four days, but which could only be rebuilt and raised to their former importance with great sacrifices of time, labor, and human life.

The rebels, however, fared badly, and had no cause to glory in their triumph. The southern tribes, whose number was four thousand souls at the outbreak of the revolt, are now reduced to four hundred, for not only was war waged against

them by the Californian and foreign militia, but they had also quarrels among themselves.* Yet these causes were less effective in their destruction than the loathsome diseases and ulcers by which they were visited, and among the four hundred that now remain, only a few are free from the general malady and enjoy the blessing of sound health.

On the other hand, be that grace of Heaven a thousand times praised, which, in our day also, inspires among the members of the Catholic priesthood, and especially in the Society of Jesus, men of superior courage who, without the slightest self-interest and for the sole purpose of propagating the Christian faith, not only brave all dangers to which they are exposed in wild countries and amidst barbarous tribes, but who also willingly give up their lives when occasion demands such sacrifices! For besides these two Californian missionaries, many others belonging to the same society have suffered death in the course of this century, while engaged in the conversion of heathen nations. Among the great number of these victims, I will only mention Father Thomas Tello, a Spaniard, and Father Henry Ruhen, a German from Westphalia, both Jesuits, who were killed as late as 1751, by the mutinous Pimas, on the other side of the Californian gulf. With Father Ruhen, I had crossed the Atlantic ocean a year before, and we made also in company the journey overland as far as the Pimeria, where he closed his days six months after his arrival.

CHAPTER VII.—THEIR TREATMENT OF THE SICK.—FUNERAL CUSTOMS.

With all their poor diet and hardships, the Californians are seldom sick. They are in general strong, hardy, and much healthier than the many thousands who live daily in abundance and on the choicest fare that the skill of Parisian cooks can prepare. It is very probable that most Californians would attain a considerable age, after having safely passed through the dangers of their childhood; but they are immoderate in eating, running, bathing, and other matters, and thus doubtless shorten their existence. Excepting consumption and that disease which was brought from America to Spain and Naples, and from thence spread over various countries, they are but little subject to the disorders common in Europe; podagra, apoplexy, dropsy, cold and petechial fevers being almost unknown among them. There is no word in their language to express sickness in general or any particular disease. "To be sick," they signify by the phrase *atemba-tíe*, which means "to lie down on the ground," though all those in good health may be seen in that position the whole day, if they are not searching for food or otherwise engaged. When I asked a Californian what ailed him, he usually said, "I have a pain in my chest," without giving further particulars.

For the small-pox the Californians are, like other Americans, indebted to Europeans, and this disease assumes a most pestilential character among them. A piece of cloth which a Spaniard, just recovered from the small-pox, had given to a Californian communicated, in the year 1763, the disease to a small mission, and in three months more than a hundred individuals died, not to speak of many others who had been infected, but were saved by the unwearied pains and care of the missionary. Not one of them would have escaped the malady, had not the majority run away from the neighborhood of the hospital as soon as they discovered the contagious nature of the disease.

In the month of April of the same year, 1763, a young and strong woman of my mission was seized with a very peculiar disorder, consisting in eructa-

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* This is the only instance in which the author alludes to wars among the natives in the body of his book, though the first appendix contains, on page 328, the following remark in refutation of a passage in the French translation of Venegas's work: "All that is said in reference to the warfare of the Californians is wrong. In their former wars they merely attacked the enemy unexpectedly during the night, or from an ambush, and killed as many as they could, without order, previous declaration of war, or any ceremonies whatever."

tions of such violent character that the noise almost resembled thunder, and could be heard at a distance of forty and more paces. The eructations lasted about half a minute, and followed each other after an interval of a few minutes. The appetite of the patient was good, and she complained of nothing else. In this condition she remained for a week, when she suddenly dropped down in such a manner that I thought she would never rise again; but I was mistaken, for the eructations and the peculiar fits continued for three years, until she became at last emaciated and died in the month of July, 1766. A few days after the outbreak of her malady, her husband was attacked by the same disorder, and on my departure, in 1768, I left him without hope of recovery. Subsequently the woman's brother and his wife suffered in like manner, and after these several other Californians, principally of the female sex. Neither the oldest of the natives, nor missionaries living for thirty years in the country, had hitherto been acquainted with this extraordinary and apparently contagious disease.

The patience of Californians in sickness is really admirable. Hardly a sigh is heaved by those who lie on the bare ground in the most pitiable condition and racked with pain. They look without dread upon their ulcers and wounds, and submit to burning and cutting, or make incisions in their own flesh for extracting thorns and splinters, with as much indifference as though the operation were performed on somebody else. It is, however, an indication of approaching death when they lose their appetite.

Their medical art is very limited, consisting almost exclusively, whatever the character of the disease may be, in the practice of binding, when feasible, a cord or coarse rope tightly around the affected part of the body. Sometimes they make use of a kind of bleeding by cutting with a sharp stone a few small openings in the inflamed part, in order to draw blood and thus relieve the patient. Though every year a number of Californians die by the bite of the rattlesnake, their only remedy against such accidents consists in tightly binding the injured member a little above the wound towards the heart; but if the part wounded by the reptile is a finger or a hand, they simply cut it off, and I knew several who had performed this cure on themselves or on individuals of their families. Now-a-days they beg in nearly all cases of disease for tallow to rub the affected part, and also for Spanish snuff which they use against headache and sore eyes. Excepting the remedies just mentioned, they have no appliances whatever against ulcers, wounds, or other external injuries, and far less against internal disorders; and though they may repeatedly have seen the missionary using some simple for removing a complaint, they will, either from forgetfulness or indolence, never employ it for themselves or others, but always apply to the missionary again.

They do not, however, content themselves with these natural remedies, but have also recourse to supernatural means, which certainly never brought about a recovery. There are many impostors among them, pretending to possess the power of curing diseases, and the ignorant Indians have so much faith in their art that they send for one or more of these scoundrels whenever they are indisposed. In treating a sick person, these jugglers employ a small tube, which they use for sucking or blowing the patient for a while, making, also, various grimaces and muttering something which they do not understand themselves, until, finally, after much hard breathing and panting, they show the patient a flint, or some other object previously hidden about their persons, pretending to have at last removed the real cause of the disorder. Twelve of these liars received one day, by my orders, the punishment they deserved, and the whole people had to promise to desist in future from these practices, or else I would no more preach for them. But when, a few weeks afterwards, that individual, who first of all had engaged to renounce the devil, fell sick, he sent immediately again for the blower to perform the usual jugglery.

It is to be feared that some of those who are seized with illness far from the mission, and not carried thither, are buried alive, especially old people, and such as have few relations, for they are in the habit of digging the grave two or three days before the patient breathes his last. It seems tedious to them to spend much time near an old, dying person that was long ago a burden to them and looked upon with indifference. A person of my acquaintance restored a girl to life that was already bound up in a deer-skin, according to their custom, and ready for burial, by administering to her a good dose of chocolate. She lived many years afterwards. On their way to the mission, some natives broke the neck of a blind, sick old woman, in order to be spared the trouble of carrying her a few miles further. Another patient, being much annoyed by gnats, which no one felt inclined to keep off from him, was covered up in such a manner that he died of suffocation. In transporting a patient from one place to another, they bind him on a rude litter, made of crooked pieces of wood, which would constitute a perfect rack for any but Indian bones, the carriers being in the habit of running with their charge.

Concerning their consciences and eternity, the Californians are perfectly quiet during their sickness, and die off as calmly as though they were sure of heaven. As soon as a person has given up the ghost, a terrible howling is raised by the women that are present, and by those to whom the news is communicated, yet no one sheds tears, excepting, perhaps, the nearest relations, and the whole proceeding is a mere ceremony. But who would believe that some of them show a dislike to be buried according to the rites of the Catholic religion? Having noticed that certain individuals, who were dangerously sick, yet still in possession of their faculties, objected to being led or carried to the mission, in order to obtain there both spiritual and material assistance, I inquired the cause of this strange behavior, and was informed they considered it as a derision of the dead to bury them with ringing of the bells, chanting, and other ceremonies of the Catholic church.

One of them told me they had formerly broken the spine of the deceased before burying them, and had thrown them into the ditch, rolled up like a ball, believing that they would rise up again if not treated in this manner. I saw them, however, frequently putting shoes on the feet of the dead, which rather seems to indicate that they entertain the idea of a journey after death; but whenever I asked them why they observed this probably very ancient custom, they could not give me any satisfactory answer. In time of mourning, both men and women cut off their hair almost entirely, which formerly was given to their physicians or conjurers, who made them into a kind of mantle or large wig, to be worn on solemn occasions.

When a death has taken place, those who want to show the relations of the deceased their respect for the latter lie in wait for these people, and if they pass they come out from their hiding-place, almost creeping, and intonate a mournful, plaintive, *hu, hu, hu!* wounding their heads with pointed, sharp stones, until the blood flows down to their shoulders. Although this barbarous custom has frequently been interdicted, they are unwilling to discontinue it. When I learned, a few years ago, that some had been guilty of this transgression after the death of a certain woman, I left them the choice either to submit to the fixed punishment or to repeat this mourning ceremony in my presence. They chose the latter, and, in a short time, I saw the blood trickling down from their lacerated heads.

CHAPTER VIII.—THEIR QUALIFICATIONS AND MANNERS.

From what I have already said of the Californians, it might be inferred that they are the most unhappy and pitiable of all the children of Adam. Yet such a supposition would be utterly wrong, and I can assure the reader that, as far as their temporal condition is concerned, they live unquestionably much

happier than the civilized inhabitants of Europe, not excepting those who seem to enjoy all the felicity that life can afford. Habit renders all things endurable and easy, and the Californian sleeps on the hard ground and in the open air just as well and soft as the rich European on the curtained bed of down in his splendidly decorated apartment. Throughout the whole year nothing happens that causes a Californian trouble or vexation, nothing that renders his life cumbersome and death desirable; for no one harasses and persecutes him, or carries on a lawsuit against him; neither a hail-storm nor an army can lay waste his fields, and he is not in danger of having his house and barn destroyed by fire. Envy, jealousy, and slander embitter not his life, and he is not exposed to the fear of losing what he possesses, nor to the care of increasing it. No creditor lays claim to debts; no officer extorts duty, toll, poll-tax, and a hundred other tributes. There is no woman that spends more for dress than the income of the husband allows; no husband who gambles or drinks away the money that should serve to support and clothe the family; there are no children to be established in life; no daughters to be provided with husbands; and no prodigal sons that heap disgrace upon whole families. In one word, the Californians do not know the meaning of *meum* and *tuum*, those two ideas which, according to St. Gregory, fill the few days of our existence with bitterness and uncountable evils.

Though the Californians seem to possess nothing, they have, nevertheless, all that they want, for they covet nothing beyond the productions of their poor, ill-favored country, and these are always within their reach. It is no wonder, then, that they always exhibit a joyful temper, and constantly indulge in merriment and laughter, showing thus their contentment, which, after all, is the real source of happiness.

The Californians know very little of arithmetic, some of them being unable to count further than *six*, while others cannot number beyond *three*, insomuch that none of them can say how many fingers he has. They do not possess anything that is worth counting, and hence their indifference. It is all the same to them whether the year has six or twelve months, and the month three or thirty days, for every day is a holiday with them. They care not whether they have one or two or twelve children, or none at all, since twelve cause them no more expense or trouble than one, and the inheritance is not lessened by a plurality of heirs. Any number beyond six they express in their language by *much*, leaving it to their confessor to make out whether that number amounts to seven, seventy, or seven hundred.

They do not know what a year is, and, consequently, cannot say when it begins and ends. Instead of saying, therefore, "a year ago," or "during this year," the Californians who speak the Waïcuri language use the expressions, *it is already an ambía past*, or, *during this ambía*, the latter word signifying the pitahaya fruit, of which a description has been given on a previous page. A space of three years, therefore, is expressed by the term "three pitahayas;" yet they seldom make use of such phrases, because they hardly ever speak among themselves of years, but merely say, "long ago," or, "not long ago," being utterly indifferent whether two or twenty years have elapsed since the occurrence of a certain event. For the same reason they do not speak of months, and have not even a name for that space of time. A week, however, they call at present *ambúja*, that is, "a house," or "a place where one resides," which name they have now, *per antonomasiam*, bestowed upon the church They are divided into bands, which alternately spend a week at the mission, where they have to attend church-service, and thus the week has become among them synonymous with the church.

When the Californians visit the missionary for any purpose, they are perfectly silent at first, and when asked the cause of their visit, their first answer is *vâra*, which means "nothing." Having afterwards delivered their speech,

they sit down, unasked; in doing which the women stretch out their legs, while the men cross them in the oriental fashion. The same habits they observe also in the church and elsewhere. They salute nobody, such a civility being unknown to them, and they have no word to express greeting. If something is communicated to them which they do not like, they spit out sideways and scratch the ground with their left foot to express their displeasure.

The men carry everything on their heads; the women bear loads on their backs suspended by ropes that pass around their foreheads, and in order to protect the skin from injury, they place between the forehead and the rope a piece of untanned deer-hide, which reaches considerably above the head, and resembles, from afar, a helmet, or the high head-dress worn by ladies at the present time.

The Californians have a great predilection for singing and dancing, which are always performed together; the first is called *ambéra diti*, the latter *agénari*. Their singing is nothing but an inarticulate, unmeaning whispering, murmuring, or shouting, which every one intonates according to his own inclination, in order to express his joy. Their dances consist in a foolish, irregular gesticulating and jumping, or advancing, retreating, and walking in a circle. Yet, they take such delight in these amusements that they spend whole nights in their performance, in which respect they much resemble Europeans, of whom certainly more have killed themselves during Shrovetide and at other times by dancing, than by praying and fasting. These pastimes, though innocent in themselves, had to be rigidly interdicted, because the grossest disorders and vices were openly perpetrated by the natives during the performances; but it is hardly possible to prevent them from indulging in their sports. While speaking of these exercises of the natives, I will also mention that they are exceedingly good runners. I would gladly have yielded up to them my three horses for consumption if I had been as swift-footed as they; for, whenever I travelled, I became sooner tired with riding than they with walking. They will run twenty leagues to-day, and return to-morrow to the place from whence they started without showing much fatigue. Being one day on the point of setting out on a journey, a little boy expressed a wish to accompany me, and when I gave him to understand that the distance was long, the business pressing, and my horse, moreover, very brisk, he replied with great promptness: "Thy horse will become tired, but I will not." Another time I sent a boy of fourteen years with a letter to the neighboring mission, situated six leagues from my residence. He started at seven o'clock in the morning, and when about a league and a half distant from his place of destination, he met the missionary, to whom the letter was addressed, mounted on a good mule, and on his way to pay me a visit. The boy turned round and accompanied the missionary, with whom he arrived about noon at my mission, having walked within five hours a distance of more than nine leagues.

With boys and girls who have arrived at the age of puberty, with pregnant women, new-born children, and women in child-bed, the Californians observed, and still secretly observe, certain absurd ceremonies of an unbecoming nature, which, for this reason, cannot be described in this book.

There existed always among the Californians individuals of both sexes who played the part of sorcerers or conjurers, pretending to possess the power of exorcising the devil, whom they never saw; of curing diseases, which they never healed; and of producing pitahayas, though they could only eat them. Sometimes they went into caverns, and, changing their voices, made the people believe that they conversed with some spiritual power. They threatened also with famine and diseases, or promised to drive the small-pox and similar plagues away and to other places. When these braggarts appeared formerly in their gala apparel, they wore long mantles made of human hair, of which the missionaries burned a great number in all newly established missions. The object

of these impostors was to obtain their food without the trouble of gathering it in the fields, for the silly people provided them with the best they could find, in order to keep them in good humor and to enjoy their favor. Their influence is very small now-a-days; yet the sick do not cease to place their confidence in them, as I mentioned in the preceding chapter.

It might be the proper time now to speak of the form of government and the religion of the Californians previous to their conversion to Christianity; but neither the one nor the other existed among them. They had no magistrates, no police, and no laws; idols, temples, religious worship or ceremonies were unknown to them, and they neither believed in the true and only God, nor adored false deities.* They were all equals, and every one did as he pleased, without asking his neighbor or caring for his opinion, and thus all vices and misdeeds remained unpunished, excepting such cases in which the offended individual or his relations took the law into their own hands and revenged themselves on the guilty party. The different tribes represented by no means communities of rational beings, who submit to laws and regulations and obey their superiors, but resembled far more herds of wild swine, which run about according to their own liking, being together to-day and scattered to-morrow, till they meet again by accident at some future time. In one word, the Californians lived, *salva venia*, as though they had been freethinkers and materialists.†

I made diligent inquiries, among those with whom I lived, to ascertain whether they had any conception of God, a future life, and their own souls, but I never could discover the slightest trace of such a knowledge. Their language has no words for "God" and "soul," for which reason the missionaries were compelled to use in their sermons and religious instructions the Spanish words *Dios* and *alma*. It could hardly be otherwise with people who thought of nothing but eating and merry-making and never reflected on serious matters, but dismissed everything that lay beyond the narrow compass of their conceptions with the phrase *aipekériri*, which means "who knows that?" I often asked them whether they had never put to themselves the question who might be the creator and preserver of the sun, moon, stars, and other objects of nature, but was always sent home with a *vára*, which means "no" in their language.

CHAPTER IX.—HOW THEY LIVED BEFORE AND AFTER THEIR CONVERSION.

I will now proceed to describe in a few words in what manner the unbaptized Californians spent their days.

In the evening, when they had eaten their fill, they either lay down, or sat together and chatted till they were tired of talking, or had communicated to each other all that they knew for the moment. In the morning they slept until hunger forced them to rise. As soon as they awakened, the eating recommenced, if anything remained; and the laughing, talking, and joking were likewise resumed. After this morning-prayer, when the sun was already somewhat high, the men seized their bows and arrows, and the women hitched on their yokes and turtle-shells. Some went to the right, others to the left; here six, there four, eight, or three, and sometimes one alone, the different bands always continuing the laughing and chattering on their way. They looked around to espy a mouse, lizard, snake, or perhaps a hare or deer; or tore up here and there a yuka or other root, or cut off some aloës. A part of the day

* According to Father Piccolo, the Californians worshipped the moon; and Venegas mentions the belief in a good and bad principle as prevailing among the Pericues and Cotchimies.—(*Waitz, Anthropologie der Naturvölker*, vol. iv, p. 250.) These statements are emphatically refuted by Baegert in his first appendix, p. 315, where he says: "It is not true that they worshipped the moon, or practiced any kind of idolatry."

† This is literally his expression.

thus spent, a pause was made. They sat or lay down in the shade, if they happened to find any, without, however, allowing their tongues to come to a stand-still, or they played or wrestled with each other, to find out who was the strongest among them and could throw his adversaries to the ground, in which sport the women likewise participated. Now they either returned to the camping-place of the preceding night, or went a few leagues further, until they came to some spot supplied with water, where they commenced singeing, burning, roasting, and pounding the captures they had made during the day. They ate as long as they had anything before them and as there was room in their stomachs, and after a long, childish or indecent talk, they betook themselves to rest again. In this manner they lived throughout the whole year, and their conversation, if it did not turn on eating, had always some childish trick or knavery for its subject. Those of the natives who cannot be put to some useful labor, while living at the mission, spend their time pretty much in the same way.

Who would expect, under these circumstances, to find a spark of religion among the Californians? It is true, they spoke of the course taken by a deer that had escaped them at nightfall with an arrow in his side, and which they intended to pursue the next morning, but they never speculated on the course of the sun and the other heavenly bodies; they talked about their pitahayas, even long before they were ripe, yet it never occurred to them to think of the Creator of the pitahayas and other productions around them.

I am not unacquainted with the statement of a certain author, according to which one Californian tribe at least was found to possess some knowledge of the incarnation of the Son of God and the Holy Trinity; but this is certainly an error, considering that such a knowledge could only have been imparted by the preachers of the Gospel. The whole matter doubtless originated in a deception on the part of the natives, who are very mendacious and inclined to invent stories calculated to please the missionary; while, on the other hand, every one may be easily deceived by them who has not yet found out their tricks. It is, moreover, a very difficult task to learn anything from them by inquiry; for, besides their shameless lies and unnecessarily evasive answers, they entangle, from inborn awkwardness, the subject in question in such a pitiable manner, and contradict themselves so frequently, that the inquirer is very apt to lose his patience. A missionary once requested me to find out whether a certain N. had been married before his baptism, which he received when a grown man, with the sister of M. A simple "yes" or "no" would have answered the question and decided the matter at once. But the examination lasted about three-quarters of an hour, at the end of which I knew just as little as before. I wrote down the questions and answers, and sent the protocol to the missionary, who was no more successful than myself in arriving at the final result, whether N. had been the husband of the sister of M. or not. So confused are the minds of these Californian Hottentots.

Of baptized Indians, there resided in each mission as many as the missionary could support and occupy with field-labor, knitting, weaving, and other work. Where it was possible to keep a good number of sheep, spinning-wheels and looms were in operation, and the people received more frequently new clothing than at other stations. In each mission there were also a number of natives appointed for special service, namely, a sacristan, a goat-herd, a tender of the sick, a catechist, a superintendent, a fiscal, and two dirty cooks, one for the missionary and the other for the Californians. Of the fifteen missions, however, there were only four, and these but thinly populated, which could support and clothe all their parishioners, and afford them a home during the whole year. In the other missionary stations, the whole people were divided into three or four bands which appeared alternately once in a month at the mission and encamped there for a week.

Every day at sunrise they all attended mass, during which they said their beads. Before and after mass they recited the Christian doctrine, drawn up for them in questions and answers in their own language. An address or exhortation delivered by the missionary in the same language, and lasting from half an hour to three-quarters of an hour, concluded the religious service of the morning. This over, breakfast was given to those who were engaged in some work, while the others went where they pleased in order to gather their daily bread in the fields, if the missionary was unable to provide them with food. Towards sunset, a signal with the bell assembled them all again in the church to say their beads and the litany of Loretto, or to sing it on Sundays and holidays. The bell was not only rung three times a day, as usual, but also at three o'clock in the afternoon, in honor of the agony of Christ, and also, according to Spanish custom, at eight o'clock in the evening, to pray for the faithful departed. When the week was over, the parishioners returned to their respective homes, some three or six, others fifteen or twenty leagues distant from the mission.

On the principal holidays of the year, and also during passion-week, all members of the community were assembled at the mission, and they received at such times, besides their ordinary food, some head of cattle and a good supply of Indian corn for consumption; dried figs and raisins were also given them without stint in all missions where such fruit was raised. On these occasions, articles of food and apparel were likewise put up as prizes for those who were winners in the games they played, or excelled in shooting at the target.

Fiscals and superintendents, appointed from among the different bands, preserved order within and without the mission. It was their duty to lead all those who were present to the church when the bell rung, and to collect and drive in to the mission that portion of the community which had been roaming for three weeks at large. They were to prevent disorders, public scandals and knaveries, and to enforce decent behavior and silence during church-service. It was further their duty to make the converts recite the catechism morning and evening, and to say their beads in the fields; to punish slight transgressions, and to report more serious offences at the proper place; to take care of those who fell sick in the wilderness, and to convey them to the mission, &c., &c. As a badge of their office they carried a cane which was often silver-headed. Most of them were very proud of their dignity, but only a few performed their duty, for which reason they received their flogging oftener than the rest, and had to bear the blows and cuffs, which it was their duty to administer to others.* There were also catechists appointed upon whom it was incumbent to lead the prayers, and to give instruction to the most ignorant of the catechumens.

Every day, in the morning, at noon, and in the evening, either the missionary himself, or some one appointed by him, distributed boiled wheat or maize to the pregnant women, the blind, old and infirm, if he was unable to feed them all; and for those who were sick, meat was cooked at least once every day. When any work was done, all engaged in it were fed three times a day. Yet their labor was by no means severe. Would to God it had been

* On a preceding page the author gives, not exactly in the proper place, the following particulars concerning the penal law established among the Californians: "In cases of extraordinary crimes, the punishment of the natives was fixed by the royal officer who commanded the Californian squadron; common misdeeds fell within the jurisdiction of the corporal of the soldiers stationed in each mission. Capital punishment, by shooting, was only resorted to in cases of murder; all other transgressions were either punished by a number of lashes administered with a leather whip on the bare skin of the culprit, or his feet put in irons for some days, weeks, or months. As to ecclesiastical punishments, the Roman pontiffs did not think proper to introduce them among the Americans, and fines were likewise out of the question, in accordance with the old German proverb: 'Where there is nothing, the emperor has no rights.'"

possible to make them work like the country people and mechanics in Germany! How many knaveries and vices would have been avoided every day! The work always commenced late, and ceased before the sun was down. At noon they rested two hours. It is certain that six laborers in Germany do more work in six days than twelve Californians in twelve days. And, moreover, all their labor was for their own or their countrymen's benefit; for the missionary derived nothing but care and trouble from it, and might easily have obtained elsewhere the few bushels of wheat or Indian corn which he needed for his own consumption.

For the rest, the missionary was the only refuge of the small and grown, the sick and the healthy, and he had to bear the burden of all concerns of the mission. Of him the natives requested food and medicine, clothing and shoes, tobacco for smoking and snuffing, and tools, if they intended to manufacture anything. He had to settle their quarrels, to take charge of the infants who had lost their parents, to provide for the sick, and to appoint watchers by the dying. I have known missionaries who seldom said their office while the sun shone, so much were they harassed the whole day. Fathers Ugarte and Druet, for instance, worked in the fields, exposed to the hot sun, like the poorest peasants or journeymen, standing in the water and mire up to their knees. Others carried on the trades of tailors and carpenters, masons, brick-burners and saddlers; they acted as physicians, surgeons, organists, and schoolmasters, and had to perform the duties of parents, guardians, wardens of hospitals, beadles, and many others. The intelligent reader, who has so far become acquainted with the condition of the country and its inhabitants, can easily perceive that these exertions on the part of the missionaries were dictated by necessity, and he will, also, be enabled to imagine in what their rents and revenues, in California not only, but in a hundred other places of America, may have consisted.

CHAPTER X.—THEIR LANGUAGE.

The account thus far given of the character and the habits of the Californians will, to a certain extent, enable the reader to form, in advance, an estimate of their language. A people without laws and religion, who think and speak of nothing but their food and other things which they have in common with animals, who carry on no trade, and entertain no friendly intercourse with neighboring tribes, that consist, like themselves, only of a few hundred souls and always remain within their own small district, where nothing is to be seen but thorns, rocks, game, and vermin, such a people, I say, cannot be expected to speak an elegant and rich language. A man of sixty years ran away from my mission with his son, a boy of about six years, and they spent five years alone in the Californian wilderness, when they were found and brought back to the mission. Every one can imagine how and on what subjects these two hermits may have conversed in their daily intercourse. The returned lad, who had then nearly reached his twelfth year, was hardly able to speak three words in succession, and excepting *water*, *wood*, *fire*, *snake*, *mouse*, and the like, he could name nothing, insomuch that he was called the dull and dumb Pablo, or Paul, by his own countrymen. The story of this boy may almost be applied to the whole people.

Leaving aside a great many dialects and offshoots, six entirely different languages have thus far been discovered in California, namely, the *Laymóna*, about the mission of Loreto; the *Cotshimi*, in the mission of St. Xavier, and others towards the north; the *Utshiti* and the *Pericúa* in the south; the still unknown language spoken by the nations whom Father *Linck* visited in 1766, during his exploration of the northern part of the peninsula; and, lastly, the *Waïcuri* language, of which I am now about to treat, having learned as much of it as was necessary for conversing with the natives.

The Waïcuri language* is of an exceedingly barbarous and rude description, by which rudeness, however, I do not mean a hard pronunciation or a succession of many consonants, for these qualities do not form the essence of a language, but merely its outward character or conformation, and are more or less imaginary, as it were, among those who are unacquainted with it. It is well known that Italians and Frenchmen consider the German language as barbarous, while the Germans have the like opinion of the Bohemian or Polish languages; but these impressions cease as soon as the Frenchmen or Italians can converse in German, and the Germans in the Bohemian or Polish tongues.

In the Waïcuri alphabet the letters *o, f, g, l, x, z* are wanting, also the *s*, excepting in the *tsh;* but the great deficiency of the language consists in the total absence of a great many words, the want of which would seem to render it almost impossible for reasonable beings to converse with each other and to receive instruction in the Christian religion. For whatever is not substantial, and cannot be seen or touched or otherwise perceived by the senses, has no name in the Waïcuri language. There are no nouns whatever for expressing virtues, vices, or the different dispositions of the mind, and there exist only a few adjectives of this class, namely, *merry, sad, lazy*, and *angry*, all of which merely denote such humors as can be perceived in a person's face. All terms relating to rational human and civil life, and a multitude of words for signifying other objects, are entirely wanting, so that it would be a vain trouble to look in the Waïcuri vocabulary for the following expressions: *life, death, weather, time, cold, heat, world, rain, understanding, will, memory, knowledge, honor, decency, consolation, peace, quarrel, member, joy, imputation, mind, friend, friendship, truth, bashfulness, enmity, faith, love, hope, wish, desire, hate, anger, gratitude, patience, meekness, envy, industry, virtue, vice, beauty, shape, sickness, danger, fear, occasion, thing, punishment, doubt, servant, master, virgin, judgment, suspicion, happiness, happy, reasonable, bashful, decent, clever, moderate, pious, obedient, rich, poor, young, old, agreeable, lovely, friendly, half, quick, deep, round, contended, more, less, to greet, to thank, to punish, to be silent, to promenade, to complain, to worship, to doubt, to buy, to flatter, to caress, to persecute, to dwell, to breathe, to imagine, to idle, to insult, to console, to live*, and a thousand words of a similar character.†

The word *living* they have neither as a noun nor as a verb, neither in a natural nor a moral sense; but only the adjective *alive. Bad, narrow, short, distant, little*, &c., they cannot express unless by adding the negation *ja* or *ra*‡ to the words *good, wide, long, near*, and *much*. They have particular words for signifying *an old man, an old woman, a young man, a young woman*, and so forth; but the terms *old* or *young* do not exist in their language. The Waïcuri contains only four words for denoting the different colors, insomuch that the natives cannot distinguish in their speech yellow from red, blue from green, black from brown, white from ash-colored, &c.

Now let the reader imagine how difficult it is to impart to the Californians any knowledge of European affairs; to interpret for them some article from a

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* *Waïcuri.* Father Begert's very curious account of the language is contained on pages 177–194 of the "Nachrichten." It comprises, besides the general remarks on the characteristic features of the language, the Lord's Prayer and the Creed, both with literal and free translations, and the conjugation of a verb.—W. W. T.—*The Literature of American Aboriginal Languages, by Hermann E. Ludewig, with Additions and Corrections, by Professor William W. Turner.* London, 1858, p. 245.

It may be remarked in this place, that the author's name is printed in three different ways, viz: *Beger, Begert*, and *Baegert*. In writing "*Baegert*," I follow Waitz, who probably gives the correct spelling of the name.

† The author adds: "And all nouns in general that end in German in *heit, keit, niss, ung*, and *schaft*."

‡ It will hardly be necessary to mention that the Waïcuri words must be pronounced as German. Excepting the *tsch*, which is replaced by the equivalent English sound *tsh*, the orthography of the author has strictly been preserved.

Madrid newspaper, if one happens to be seen in California a year or more after its appearance; or to enlarge upon the merits of the Saints, and to explain, for instance, how they renounced all vanity, forsaking princely possessions and even kingdoms, and distributed their property among the poor; how their lives were spent in voluntary poverty, chastity, and humility; and, further, that they subjected themselves for years to the severest penances, conquered their passions and subdued their inclinations; that they devoted daily eight and more hours to prayer and contemplation; that they disregarded worldly concerns and even their own lives; slept on the bare ground, and abstained from meat and wine. For want of words, the poor preacher has to place his finger to his mouth in order to illustrate eating; and concerning the comforts of life, every Californian will tell him that he never, as long as he lived, slept in a bed; that he is entirely unacquainted with such articles as bread, wine, and beer; and that, excepting rats and mice, he hardly ever tasted any kind of meat.

The above-mentioned and a great many other words are wanting in the Waïcuri language, simply because those who speak it never use these terms; their almost animal-like existence and narrow compass of ideas rendering the application of such expressions superfluous. But concerning *heat* or *cold*, *rain* or *sickness*, they content themselves by saying, *it is warm*, *it rains*, *this* or *that person is sick*, and nothing else. Sentences like the following: "The sickness has much weakened a certain person;" or, "cold is less endurable than heat;" or, "after rain follows sunshine," &c., are certainly very simple in themselves and current among all peasants in Europe, yet infinitely above the range of thought and speech of the Californians.

They cannot express the degrees of relationship, for instance, *father*, *mother*, *son*, *brother*, nor the parts of the human body, nor many other words, such as *word* or *speech*, *breath*, *pain*, *comrade*, *&c.*, singly and without prefixing the possessive pronouns *my*, *thy*, *our*, *&c.* They say, therefore, *bedáre*, *edáre*, *tiáre*, *kepedáre*, *&c.*, that is, *my*, *thy*, *his*, *our father;* and *bécue*, *écue*, *ticue*, *kepécue*, that is, *my*, *thy*, *his*, *our mother*. So also *mapà*, *etapà*, *tapà*, that is, *my*, *thy*, *his forehead*. *Minamù*, *einamù*, *tinamù*, that is, *my*, *thy*, *his nose;* *betanía*, *etanía*, *tishanía*, *my*, *thy*, *his word*; *menembeû*, *enembeû*, *tenembeû*, *my*, *thy*, *his pain*, *&c.* But no Californian who speaks the Waïcuri is able to say what the words *are*, *cue*, *apà*, *namù*, *tanía*, and *nembeû*, express, for *father*, *forehead*, *word*, or *pain* are significations which they never thought of using in a general sense, and far less has it ever entered their minds to speak, for instance, of the duties of a father, of a gloomy, a serene, a narrow or large forehead, or to make a long, a flat or an aquiline nose the subject of their conversation.

The Waïcuri language is exceedingly deficient in prepositions and conjunctions. Of the first class of words, there exist only two that have a definite application, namely, *tina*, *on* or *upon*, and *déve* or *tipítsheû*, which is equivalent to the phrase *on account of* or *for* (propter.) The prepositions *out*, *in*, *before*, *through*, *with*, *for* (pro,) *against*, *by*, *&c.*, are either represented by the words *me*, *pe*, and *te*, which have all the same meaning, or they are not expressed at all. The article is entirely wanting, and the nouns are not declined. The conjunction *tshie*, *and*, is always placed after the words which it has to connect; the other conjunctions, such as *that*, *but*, *than*, *because*, *neither*, *nor*, *yet*, *as*, *though*, *&c.*, are all wanting, and likewise the relative pronouns *which* and *who*, so frequently occurring in other languages. They have no adverbs derived from adjectives, and hardly any of the primitive class. The comparative and superlative cannot be expressed, and even the words *more* and *less* do not exist, and instead of saying, therefore, *Peter is taller and has more than Paul*, they have to use the paraphrase, *Peter is tall and has much, Paul is not tall and has not much.*

Passing to the verbs, I will mention that these have neither a conjunctive nor a mandative mood, and only an imperfect optative mood, and that the passive form is wanting as well as the reciprocal verb, which is used in the Spanish and French languages. The verbs have only one mood and three tenses, viz., a present, preterit, and future, which are formed by affixing certain endings to the root of the verb, namely, in the present *re* or *reke;* in the preterit *rikiri, rujére, raúpe,* or *raúpere;* in the future *me, méje* or *éneme.**

Sometimes the natives prefix the syllable *ku* or a *k* alone to the plural of the verb, or change its first syllable into *ku;* for example, *piabakè,* to fight, *umutù,* to remember, *jake,* to chat; but *kupiàbake, kumutù,* and *kuáke,* when they will indicate that there are several persons fighting, remembering, or chatting. A few of their verbs have also a preterit passive participle; for example, *tshipake,* to beat, *tshipitshürre,* a person that has been beaten, plural *kutipaü.* Some nouns and adjectives are likewise subject to changes in the plural number, as, for instance, *ánaï,* woman, *kánaï,* women; *entudità,* ugly or bad, and *entuditámma,*† bad or ugly women. *Be* expresses *I, me* (mihi,) *me* (me) and *my; ei* means *thou, thee* (tibi,) *thee* (te) and *thy,* and so on through all the personal and possessive pronouns. Yet *becún* or *beticún* signifies also *my,* and *ecún* or *eiticún, thy.*

They know nothing of metaphors, for which reason the phrase *blessed is the fruit of thy womb* in the "Hail Mary" has simply been replaced by *thy child.* On the other hand they are very ingenious in giving names to objects with which they were before unacquainted, calling, for instance, the door, *mouth;* bread, *the light;* iron, *the heavy;* wine, *bad water;* a gun, *bow;* the functionaries of the mission, *bearers of canes;* the Spanish captain, *wild* or *cruel;* oxen and cows, *deer;* horses and mules *titshénu-tshà,* that is, *child of a wise mother;* and the missionary, in speaking of or to him, *tià-pa-tù,* which means *one who has his house in the north, &c.*

In order to converse in such a barbarous and poor language, a European has to change, as it were, his whole nature and to become almost a Calfornian himself; but in teaching the natives the doctrines of the Christian religion in their own language, he is very often compelled to make use of paraphrases which, when translated into a civilized language, must have an odd and sometimes even ridiculous sound to Europeans; and as the reader may, perhaps, be curious to know a little more of this peculiar language, I will give as specimens two articles from the Waïcuri catechism, namely, *the Lord's Prayer* and the *Creed,* each with a double interpretation, and also the whole conjugation of the verb *amukiri.*‡

Concerning this Californian Lord's Prayer and Creed and their interpretations, the reader will take notice of the following explanatory remarks:

1. The first translation, which stands immediately under the Californian text, is perfectly literal and shows the structure of the Waïcuri language. This version must necessarily produce a bad effect upon European ears; whereas the second translation, which is less literal and therefore more intelligible, may serve to convey an idea how the Waïcuri text sounds to the natives themselves as well as to those who understand their idiom, and have become accustomed, by long practice, to the awkward position of the words, the absence of relative pronouns and prepositions, and the other deficiencies of the language.

* From the conjugation of the verb *amukiri,* given at the end of this chapter, it is evident that these endings have no reference to the person or number of the tenses, but may be indifferently employed.

† This compound word illustrates well the polysynthetic character of the Waïcuri language.

‡ We cannot be too thankful to Father Baegert, who, with all his oddity and eccentricity, has had the philological taste to preserve and explain a specimen of the Waïcuri—a favor the greater, as neither Venegas nor the polished Clavigero has preserved any specimen of a Californian language, much less a verb in full.

2. The words *holy, church, God, ghost, communion, grace, will, cross, virgin, name, hell, kingdom, bread, trespass, temptation, creator, forgiveness, life, resurrection, Lord, daily, Almighty, third, &c.*, are wanting in the Waïcuri language, and have either been paraphrased, when it was feasible, or replaced by corresponding Spanish words, in order to avoid too lengthy and not very intelligible sentences. Some words that could be omitted without materially changing the sense, such as *daily* in the Lord's Prayer, and *Lord* in the Creed, have been entirely dropped.

3. The sentence "he shall come to judge the living and the dead" could not be literally translated, because the Californians are unable to comprehend the moral and theological sense of that passage and others of similar character. Nor could they be taught in the Creed that the flesh will live again, for by "flesh" they understand nothing but the meat of deer and cows. They would laugh at the idea that men were also flesh, and consequently be led to believe in the resurrection of deer and cows, when they were told that the flesh will rise again on the day of judgment.

4. In the Waïcuri language *Heaven* is usually called *aëna*, that is, *the above;* and also, but less frequently, *tekerekádatemba*, which means *curved* or *arched earth* or *land*, because the firmament resembles a vault or arch. *Hell* they have been taught to call *the fire that never expires;* but this expression is not employed in the Waïcuri Creed.

The Lord's Prayer in the Waïcuri language, with a literal translation, showing the exact succession of the words.

Kepè-dáre tekerekádatembà daï, eï-ri akátuikè-pu-me, tshákarrake-
Our Father arched earth thou art, thee O! that acknowledge all will, praise
pu-me ti tshie: ecùn *gracia*—ri atúme catè tekerekádatembà tshie; eï-
all will people and: thy grace O! that have will we arched earth and; thee
ri jebarrakéme ti pù jaûpe datembà, páe eï jebarrakére, aëna kéa; kepecùn búo
O! that obey will men all here earth, as thee obey, above are; our food
kepe kên jatúpe untâiri; cate kuitsharrakè têi tshie kepecùn atacámara, pàe kuitsharrakère
us give this day; us forgive thou and our evil, as forgive
catè tshie cávape atukiàra kepetujakè; catè tikakambà têi tshie cuvumerà catè uē
we also they evil us do; us help thou and desire will not we anything
atukiàra; kepe kakunjà pe atacára tshie. *Amen.*
evil; us protect from evil and. Amen.

The same in a less literal translation.

Our Father, Thou art in the Heaven; O that all people may acknowledge and praise Thee! O that we may have Thy grace and Heaven! O that all men may obey Thee here in the world as obey Thee who are above! Our food give us on this day, and forgive us our sins, as we also forgive those who do us harm; and help us that we may not desire anything sinful, and protect us from evil. Amen.

The twelve articles of the Creed literally translated.

Irimánjure pè *Dios* Tiare uretì-pu-puduéne, tàupe me buarà uretírikíri
I believe in God his Father make all can, this of nothing has made
tekerekádatembà atembà tshie. Irimánjure tshie pe *Jesu Christo* titshánu íbo te
arched earth earth and. I believe also in Jesus Christ his son alone —
tiáre, éte punjére pe *Espiritu Santo*, pedára tshie me *Santa Maria virgen.*
his father's, man made by Holy Ghost, born and of Saint Mary virgin.
Irimánjure tshie tâu-vérepe *Jesu Christo* híbitsherikíri tenembeû apánne iebitshéne
I believe also this same Jesus Christ suffered has his pain great commanding
témme pe *Judea Pontio Pilato;* kutikūrre rikiri tína *cruz*, pibikíri, kejenjùta rikíri
being in Judea Pontius Pilate; extended been on cross, has died, under earth buried is
tshie; keritshéu atembà búnju; meakúnju untáiri tipè-tshetshutipè rikíri; tshukíti
also; gone down earth below; three days alive again has been; gone up
tekerekádatembà, penekà tshie me titshukotà te *Dios* tiàre uretì-pu-puduéne,
arched earth, sits also his right hand of God his father make all can,

aipéreve tenkíe meúri-ku-méje atacámma atacámmara ti tahie. Irimánjure pe
from thence reward give come will good bad men also. I believe in

Espiritu Santo; irimánjure epi *Santa Iglesia catholica,* communion te kunjukaráu
Holy Ghost; I believe there is Holy Catholic Church, communion — washed

ti tahie. Irimánjure kuitsharakéme *Dios* kumbáse-didi-re, kutéve-didi-re ti tahie
people also. I believe forgive will God hate well, confess well men and

kirun atacámmara pánne pu. Irimánjure tahie tipè tahetshutipé me tibikíu ti pù:
their bad great all. I believe and alive again will be dead people all:

enjéme típe dèi méje tucáva tahie. *Amen.*
then alive ever will be the same also. Amen.

The same less literally translated.

I believe in God the Father, who can make everything; he has made of nothing Heaven and earth. I believe also in Jesus Christ, the only Son of his Father; was made man by the Holy Ghost; was born of the Virgin Mary. I believe also this same Jesus Christ suffered great pain while Pontius Pilate was commanding in Judea: he was extended on the cross; he died and was buried; he went below the earth; he became alive again in three days: he went up to Heaven; he sitteth at the right hand of God his Father, who can make everything; he will come from thence to give rewards to the good and bad. I believe in the Holy Ghost; I believe there is a Holy Catholic Church and communion of the baptized. I believe God will forgive those men who thoroughly hate and thoroughly confess all their great sins. I believe also all dead men will become alive again, and then they will be always alive. Amen.

CONJUGATION OF THE VERB AMUKÍRI, TO PLAY.

Present.

Sing.	bè	I	play, &c.
	ei	thou	amukiri—re
	tutaû	he	
Plur.	catè	we	
	petè	you	
	tucáva	they	

Preterit.

Sing.	bè	I	have played, &c.
	ei	thou	amukiri—rikíri
	tutaû	he	*or* —rujére, —râupe, —ráûpere
Plur.	catè	we	
	petè	you	
	tucáva	they	

Future.

Sing.	bè	I	will play, &c.
	eï	thou	amukiri—me
	tutâu	he	*or* —méje, —éneme
Plur.	catè	we	
	petè	you	
	tucáva	they	

Imperative.

Sing. amukiri tei, play thou.
Plur. amukiri tu, play you.

Optative.

Sing.	bè—ri	amukiri—rikirikára	Would to God, I, thou, he, we, you, they had not played!
	ei—ri	*or*—rujerára	
	tutâu—ri		
Plur.	catè—ri		
	petè—ri		
	tucáva—ri		

APPENDIX.

*Note on the Cora and Waicuri languages, by Francisco Pimentel.**

Father Ortega refers in various places to the grammar of the Cora language which he intended to write; but the work, if it was ever written, has been lost, since there is no mention of it, and it is unknown to bibliographers.

The Cora dialect is known also by the names of Chora, Chota, and Nayarita. This last name comes from the fact that it was spoken, and is still so, in the mountains of Nayarit in the State of Jalisco. There is another idiom called Cora in California, which is a dialect of the Guaicura or Vaicura, differing from that spoken in Jalisco. I have compared various words of the Guaicura and the Cora of Jalisco, and have found them entirely different.

Examples.

	Cora.	Vaicura.
Father	Tiyaoppa	Are.
Thou art	Petehbe	Daí.
All	Manaicmic	Pu.
Man	Tevit	Ti.
And	Acta	Tschie.
Here	Yye	Taupe.
Earth or world	Chianacat	Datemba.
Above	Mehtevi	Aena.
Food	Gueahti	Bue.
To give	Ta	Ken.
Day	Xeucat	Untairi.
To pardon	Ataouniri	Kuitscha.
How	Eupat	Pae.
Obedient	Teatzahuateacame	Tebarrakere.
No	Ehe	Ra.
Something	Titac	Ue.
I	Neapue, nea	Be.
Thou	Apue, ap	Eí.
He	Aehpu, aehp	Tutau.
We	Ytean	Cate.
You	Ammo, an	Peti.
They	Aehmo, aehm	Tucava.
My	Ne	Be, me, mi, m.
Thy	A	Ei, e, et.
His	Ana, hua	Ti, te, t.
Our	Ta	Kepe.
For	Keme	Deve.
Upon	Apoan	Tina.
Game	Muaitec	Amukiri.
Son	Tiperie, tiyaoh	Tschanu.
Nose	Tzoriti	Namu.

NOTE RELATIVE TO THE AUTHOR.—The only facts concerning the author, which I was able to obtain while engaged in translating his work, are contained in *De Backer's Bibliothèque des Ecrivains de la Compagnie de Jésus, Liège* 1859. Vol. v, p. 28.

The author, whose name is given here as Jacob *Begert*, was born (1717) at Schlettstadt (Upper Rhine.) He went to California in 1751 and preached the Gospel there till the decree of Charles III tore the Jesuits from their missions. On returning to Europe, he retired to Neuburg in Bavaria, where he died in the month of December, 1772. Clavigero stands as authority for ascribing the "Nachrichten" to him, and it is also mentioned that the "Berlin'sche litterarische Wochenblatt," (1777, vol. ii, p. 625,) contains an extract of the work. Meusel's large work on German authors, entitled "Das gelehrte Deutschland," is given as the source from which these statements are derived.

The "Nachrichten" appeared first in print in 1772, the same year in which the author died, who consequently could have survived the publication of his work only a short time. The copy in my hands, which was printed in 1773, is not properly a second edition, but merely a reprint, in which the most glaring typographical errors are corrected.

* Boletin de la Sociedad Mexicana de Geografia y Estadistica. Mexico, 1862, tomo viii, num. 11, p. 603, &c.

THE FIRST STEPS

IN THE

STUDY OF HIGH ANTIQUITY IN EUROPE.

BY A. MORLOT.

FOR THE SMITHSONIAN INSTITUTION.

It is well known that in most countries of Europe there are found axes, wedges, hammers, knives, arrow-heads, &c., all of stone. The axes and wedges of stone are often called by the common people "thunderbolts," (a designation which has also been applied to certain fossils, such as the belemnites;) and it is curious to meet with that notion, not only in Brazil, where it was probably introduced by the Portuguese, but even among the Malays* and the Javanese, whilst the Japanese and the Chinese revere these objects as relics of their ancestors.† The truth as to their meaning seems to have dawned but later in Europe. Thus when, in 1734, the antiquary and numismat, Mahudel, read to the Academy of Paris a paper "on the so-called thunderbolts," showing that they were the first instruments used by man, he appears to have been reproached for "not giving the reasons, which prove the impossibility of such stones being formed in the clouds." In 1758 there appeared a remarkable work by Goguet, on the origin of the laws, arts, and sciences, in which the matter is treated in a perfectly rational manner.‡ In the preface the author lays down a principle which deserves to be given textually: "When I met with an almost total absence of facts and historical monuments, particularly for the first ages, I consulted what the authors tell us of the customs of savage nations. I thought that the habits of those people would furnish sure and correct information concerning the state of the first tribes." Further on, (chap. iv, book II,) Goguet, pointing to the stone axes and other objects of the same sort found in Europe, recognizes them as similar to those of the savages, and as having been used by our ancestors, before the latter had become acquainted with metal. He then goes on to speak of the weapons, instruments, and ornaments of copper (bronze) met in certain old graves in England, Switzerland, Germany, and chiefly in the north, and he comes to the conclusion that copper (bronze) has been used instead of iron, *(a tenu lieu du fer,)* which he finds confirmed by the most ancient historical traditions. Lastly, he points out that iron could only have been known and used later, because its common ores are not distinguished by any striking appearance, and because this metal is much more difficult to produce than copper, which is easily smelted. But Goguet was in advance of his age, and his valuable archæological remarks were lost to his contemporaries.

Later came M. de Caumont, who treated the matter in a first-rate manner. He perceived that stone implements had been the earliest in use, and that copper

* *George Windsor Earl.* "The Native Races of the Indian Archipelago," London, 1853; and "Journal of the Indian Archipelago," vol. v, p. 84.

† *Von Siebold.* "Archief vor Beschrijving of Japan," quoted by Earl.

‡ *A. Y. Goguet.* "De l'Origine des Lois, des Arts et des Sciences," sixth edition: Paris, 1820.

and bronze had been introduced before iron. Borrowing, according to his own acknowledgment, from the language of geologists, he introduced the happy expression of *chronological horizons*, to indicate "the periods in the history of art remarkable for revolutions or for notable changes in the forms and the character of the monuments." Without pretending to lay down a general rule, allowing of no exception, and acknowledging that he owed the idea to Sir R. Hoare, M. de Caumont also pointed out the following order of succession in the mode of burial: In the most ancient graves the body of the deceased has been doubled up, so as to bring the knees in contact with the chin, (as if to take up as little room as possible.) Later (during the bronze age) the dead were usually burnt, (which leads to infer the worship of fire.) Lastly, the body was often laid in the grave, stretched out at full length. But, taken up by his vast researches on the Roman remains, M. de Caumont treated of the older periods only in the first volume of his lectures on monumental antiquities—a work which is in itself a monument of fame for its author.*

It was reserved to the Scandinavian North definitely to open the proper track. Denmark and the south of Sweden teem with antiquities strewed in the ground, and with ancient barrows or tombs, raised like hillocks above the surrounding level. These drew attention the more readily, as Roman civilization had not penetrated so far, and had not left those traces which, for a long time, exclusively fastened research in the more southern regions of Europe.

At Copenhagen, the Athens of the Scandinavian North, many began already, in the past century, to collect those axes and wedges of flint, which attract the eye by their perfect state of preservation, and by their natural lustre. It was a current idea among men of learning that these objects were symbols and implements used in the religious ceremonies and practice of the ancient heathens. It was not ill-conceived. But imagination cannot replace induction. Hence it was an event of note when a simple merchant, M. Thomsen, whose talent of observation and rare practical tact, revealed by his ability in collecting coins, published in 1832 a paper on the antiquities of stone in the north,† showing that these objects had been the tools and weapons of a people very like the modern savages, who are not acquainted with the use of metal.

The activity of Mr. Thomsen remained concentrated on the improvement of the museum, (Oldnordisk Museum,) which was confided to his care. Thanks to him, that establishment has grown to be what the Danes may well claim as a monument of national fame. With the view of furthering the interests of that museum, Mr. Thomsen published in 1836, again anonymously, a small practical guide for the study and preservation of northern antiquities.‡ The book deserves particular notice, as being the first in which the chronological classification, formerly indicated by Goguet, and even hinted at by the Latin poet Lucretius, (*De Rerum Natura*, lib. v,) is expressly laid down and actually carried out. In a chapter on the different periods to which the Pagan antiquities may be referred, Mr. Thomsen begins by speaking of the implements of stone, of which he had already treated in his first paper. He then shows that certain sepulchral chambers, formed of huge boulders, and in which the dead were deposited without being burned, contain the same stone implements, without any traces of metal. This furnishes him with his first period, which he calls *the stone age*. The author then goes on to show, as Goguet had already done, that copper and bronze must have been in use before the iron, and he points out how cutting-

* *De Caumont.* "Cours d'Antiquités Monumentales, professé a Caen," 6 vols., with atlas of plates: vol. 1: Paris, 1836.

† "Nordisk Tidsskrift for Oldkyndighed," vol. 1: Copenhagen, 1832. Eighteen pages in octavo, with three plates. Anonymous.

‡ "Ledetraad til Nordisk Oldkyndighed." Copenhagen, 1836. German edition: "Leitfaden zur Nordischen Atterthumskunde." Copenhagen, 1837. English edition, by Lord Ellesmere: "A Guide to Northern Antiquities." London, 1848.

implements and weapons of bronze alone, without any iron, are found in certain graves, differing from those of the preceding period, both by their structure and also by their dead having been burned. Hence he deduces a second period, which he calls *the bronze age*. Next comes *the iron age*, distinguished also by a new system of burial and by the first appearance of silver, which was wanting in the bronze age, though the latter already worked gold. Thus, what iron is now, and has long been, for industry and civilization in general, bronze was formerly, and stone was still earlier. Mr. Thomsen also points out, in his Guide, that no traces of alphabetical inscriptions occur before the appearance of iron, and that each of the three periods is distinguished by its peculiar style of ornament.

While these labors were being prosecuted in Denmark, others not less important were undertaken in Sweden. Wm. S. Nilsson, professor of zoology at the flourishing University of Lund, began the publication of a great work on the fauna of Scandinavia. Considering his subject from a comprehensive point of view, Professor Nilsson included in it man himself and his origin. This called his attention to the flint implements, and he formed a collection of them, constituting now the chief ornament of the museum of Lund. He published his archæological researches first as a chapter on the history of the chase and fishery in the Scandinavian North, inserted in the first volume of his Fauna, (Lund, 1835,) and later, with more ample details, in a separate quarto volume, entitled "The Aborigines of the Scandinavian North, a treatise of Comparative Ethnography and a contribution towards the History of the Development of Humanity." This work, comprising 280 figures, appeared at Lund, in four parts, from 1838 to 1843. The author handles his subject with all the superiority of real genius, expressing thought, deep and rich, in a style characterized by noble simplicity, often verging on the sublime. The illustrious Swede begins by showing that the comparative method of the naturalist must be applied to the study of the prehistoric ages, just as has been done, when the geologist compared the extinct creations with our present organic world. He then applies that method, not in a general manner, as had been done before, but entering into all the details required by serious scientific research. He compares, one by one, the flint implements of the North with those of the savages. He also points out the striking analogy between the most ancient graves in Sweden and the modern huts of the Greenlanders, with a view to prove that the abodes of the dead were imitated from the dwellings of the living, the primitive type of which seems to have been preserved to this day in Greenland. Remarking, that an ancient race cannot be determined by the shape of its weapons and tools, nor even by the style of its graves, but only by its osteological characteristics, Professor Nilsson takes a review of the skulls, and he shows that the type of the aborigines is still reproduced by the Laplanders, whose ancestors seem to have once held the whole North. He finally confirms this by a very curious inquiry into the traditions and myths of the North, applying here, also, the principle of comparison, and showing, for example, how the arrival of the first Europeans had given rise, among the Esquimaux, to similar tales.

The work in question, as its title proclaims, treats only of the primitive period, marked by the total absence of all metal, and it contains only a few passing allusions to the later periods.

In 1844 Professor Nilsson published at Lund a paper "On the successive periods of human development in Scandinavia, during the prehistorical ages." In this treatise, which is quite as remarkable as the first, although of much less extent, the three ages—of the stone, the bronze, and the iron—are at once recognized as established, and the author enters, respecting each of them, into a mass of details, which constitute the main body of the archæological principles, were then current in the North. Thus, when speaking of the bronze age, the

author points out the striking uniformity of the bronze weapons in different parts of Europe, and this leads him to the conclusion that the civilization of that period must have spread from one and the same centre, situated, probably, somewhere on the borders of the Mediterranean. The author further remarks, that the introduction of bronze, as also, later, the introduction of iron, coinciding with an essential change in the mode of burial, betrays a profound change, each time, in the religious system. Hence the conclusion, that each of those periods was marked by the invasion of a new race, or, to use the author's own terms, by a fresh wave of population; for we can hardly imagine that nations would be brought to change their religion, simply because they had acquired a new metal.

The considerable amount of labor required by the publication of his Scandinavian fauna obliged Professor Nilsson to abandon the field of archæological inquiry. But he had laid the broad and solid foundations of that combination of researches into the past and present state of mankind, which deserves to be acknowledged as a science of its own, under the denomination he proposed, of *Comparative Ethnography*. Nilsson has achieved in this branch what Cuvier has done for palæontology, when he applied his principles of comparative anatomy to the study of fossil bones.* These two great men have both developed and applied the true method; and this is much more important than any brilliant discovery, a good method being the most powerful instrument of discovery, as Cuvier himself remarked.

To the Swede Nilsson and to the Dane Thomsen, happily both still among the living, we are thus indebted for a good method, bringing archæology within the pale of natural science, and for a practical classification, based on the form and matter, and on the use of the relics of the past—that is, on positive facts, relative to industry and arts.

The classification into three ages—of the stone, the bronze, and the iron—recalls to mind the distinction established by Werner and his contemporaries of the geological formations, into Primary, Secondary and Tertiary. It has been equally useful, for it began to introduce order into the chaos of antiquities of all ages, thrown indiscriminately together in the museums, so as to cause these to look more like curiosity-shops than like scientific establishments.

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* Retired from his professorship, Mr. Nilsson has again taken up archæology. He is now publishing a new and much enlarged edition of his Scandinavian Aborigines. A German translation of this new edition is appearing at Hamburg.

PRIZE QUESTIONS.

EXTRACT FROM THE PROGRAMME OF THE HOLLAND SOCIETY OF SCIENCES AT HARLEM, FOR 1865.

The Society held its 113th annual session 20th May, 1865. Since the annual session of 1864 the society has published the following volumes of its acts:

1. Vol. xix, 2d part, H. R. Göppert, *Über Einschlüsse im Diamant.*
2. Vol. xx, 1st part, P. Bleeker, *Description de quelques espèces de Cobitioides et de Cyprinoides de Ceylon.*
3. Vol. xx, 2d part, P. Bleeker, *Description des espèces de Silures de Suriname, conservées aux Musées de Leide et de Amsterdam.*
4. Vol. xxi, 1st part, Dr. Hermann Vogelsang, *Die Vulkane der Eifel in ihrer Bildungsweise erläutert.*
5. Vol. xxi, 2d part, P. Duchassaing de Fonbressin et Giovanni Michelotti, *Spongiaires de la mer Caraïbe.*
6. Vol. xxii, 1st part, Joseph Barnard Davis, *On Synostotic Crania among Aboriginal Races of Man.*

It has been decided that the memoir presented by M. J. Beissel de Borcette, *Die Bryozoen der Aachner Kreidebildung*, shall form part of the acts of the society.

In order to give greater publicity to the scientific labors of the savants of the Netherlands, the Society have decided to publish, at its own expense, in the French language, (or in Latin, for descriptive systematic memoirs,) the *Netherland Archives of the Exact and Natural Sciences, published by the Holland Society of Sciences at Harlem:*

The publication of this journal, which will contain as well original memoirs *in extenso* as translations or summaries of the memoirs of learned Netherlanders which have appeared elsewhere, will not be periodical, but will be regulated according to the number of memoirs presented. It will be edited by the perpetual secretary, M. E. H. von Baumhauer, assisted by MM. R. von Rees, J. von der Hoeven, H. J. Halbertsma, and D. Bierens de Haan.

The Society thinks proper to repeat the following questions, and requests that they be answered *before the first of January*, 1867:

1. The fishes of the Indian Archipelago have engaged the researches of a learned Hollander. The Society desires that the other vertebrata of those islands, especially those of Borneo, Celebes, and the Moluccas, and, above all, those of New Guinea, should be the subject of a like examination. It will award its gold medal to the naturalist who shall send it either the description of some new species of mammifers, birds or reptiles of those islands, or a memoir containing new and remarkable facts regarding the structure and mode of life of some of those animals.

2. The Society desires as exact a determination as possible of the errors of the tables of the moon which we owe to M. Hansen, by the occultations of the Pleiades, observed during the last revolution of the node of the lunar orbit.

3. The celebrated mechanician Ruhmkorff has obtained sparks of extraordinary length by the machines of induction which bear his name. The Society desires to obtain a determination, by theoretical and experimental researches, of the laws which govern the length and intensity of the sparks in machines of different size and construction.

4. What difference is there between the perception of sounds with one and with both ears? Precise researches are requested regarding this difference, and on the influence in general of the duality in the organ of hearing.

5. According to the researches of M. Pasteur and other savants, fermentation is owing to the development of cryptogams and infusoria. The Society wishes new and positive researches to be made on this subject, and, if necessary, an exact description to be given of these plants and animals, and their mode of action.

6. What is the best construction for steam vessels designed to clear rivers of the masses of ice which obstruct the course of the water? It is expected that in answering this question notice will be taken of all that practice has decided upon this subject, as well in our own country as abroad.

7. With the exception of some formations on the eastern frontier of the kingdom of the low countries, the geolgoical strata of that region covered with deposits of alluvium and diluvium are still but little known. An account is desired of all that has been brought to light with certainty, whether by borings executed at different places or by other observations, respecting the nature of those formations.

8. It is known, chiefly by the labors of M. Roemer, at Breslau, that many of the fossils which are found near Groningen belong to the same species with those occurring in the silurian formations of the island of Gothland. This has led M. Roemer to the conclusion that the diluvium of Groningen has been transported from the island just named; but such an origin appears to comport but little with the direction in which this diluvium is deposited—a direction which would rather indicate a transportation from the southern part of Norway. The Society desires to see this question decided by an exact comparison of the fossils of Groningen with the minerals and fossils of the silurian and other formations of that part of Norway, with a regard at the same time to the modifications which the conveyance from a remote country and its consequences may have caused those minerals and fossils to undergo.

9. The combustion of steel, iron, and other metals in oxygen is accompanied by the apparition of a multitude of incandescent particles thrown off from the surface of the body in combustion, and which are found after the phenomenon at the bottom of the vessel in which the combustion is effected. The same fact is observed in the luminous electric arch of a strong battery between two metallic rheophores, one of which at least is of iron or steel. The Society asks an explanation, based upon new and decisive researches, of the cause of this phenomenon.

10. We request a continuation of the remarkable researches of M. Brewster on the liquids and gases which fill the small cavities sometimes found in crystallized minerals.

11. The Society invites an exact anatomical comparison between the skeleton of the *Cryptobranchus japonicus* and that of the fossil salamanders of Oeningen, as well as that of the salamander of Roth.

12. An exact description, with figures, of the skeleton and muscles of the *Sciurus vulgaris*, compared with what is known on this subject of the *Lemurides* and *Chiromys*, is requested, in order that the place to be assigned to this last species in the natural classification may be determined with more certainty than has been heretofore possible.

13. The Society wishes an anatomical description, with figures, of the American Potto, (*Cercoleptes Illiger, Vivera candivolvula Pallas,*) compared with the structure of other mammifers, as the *Nasua* and *Procyon*, and with that of the quadrumana. The attention of the author is especially directed to the communication of Mr. Owen, (Proceedings of Zoological Society 1835, p. 119–124.)

14. The physiological action of carbonic acid on the animal organism, especially that of man, is recommended as the object of new and decisive experiments.

15. It is desirable that exact researches should be instituted respecting the distribution of plants and animals in the coal strata of different countries.

16. A memoir of Savart, communicated as an extract in the Journal of the Institute, No. 313 and 314, by M. Masson, contains the account of experiments on the simultaneous movement of two pendulums. The Society would wish these experiments to be repeated and explained, if their result be confirmed.

17. Late researches on beats and the resulting sounds do not seem to lead necessarily to an abandonment of the principle of Daniel Bernouilli, of the coexistence of small oscillations for vibrations, which are propagated in an indefinite medium. The case is wholly different when the question relates to vibrations with fixed modes and centres in a circumscribed medium. The Society invites new researches, as well experimental as theoretical, on the resulting sounds and the vibrations of stretched cords and membranes which produce them by their combinations and their interference.

18. It seems that the phenomenon, known as Porrets's, of the electrical translation of matter across a porous diaphragm, is not an isolated fact, but that by considering it in its relations with other phenomena it will be found to be a particular case of a general action. New researches therefore are invited respecting this phenomenon, especially with reference to those of M. Hittorff, on the translation of elements in the electrolysis.

19. Recent researches have evinced an important analogy between the conducting power of bodies for heat and for the electric current. The substances compared in this respect were all good conductors of electricity. The Society desires researches on the conducting power as regards heat of certain isolating or slightly conducting substances, as glass, marble, &c., and a discussion of the results obtained, in reference to what has been disclosed by the remarkable researches of M. Gaugain respecting the conducting power and inductive capacity of such substances for electricity.

20. New experimental researches are requested respecting the interior friction or viscosity of liquids in movement, and on their friction against the walls of the channel or tube in which they are moving. It is particularly desirable that a viscid liquor, for instance a thick oil, should be experimentally studied under this point of view.

21. The Society solicits further exact researches upon the remarkable phenomena of dissociation discovered by M. Sainte-Claire-Deville.

For this year the following questions, whose term expires the *first of January*, 1867, have been proposed:

1. An exact determination of the heat produced by the combustion of the glycerides is desired.

2. The fractional distillation and fractional precipitation, extolled as means for separating from one another bodies homologous in their mixtures, are, as such, very insufficient. The Society invites a research for better means of obtaining that result.

3. It seems that the diminution of temperature in the successive strata of the atmospheric air is not equal under different latitudes; it is desirable to have this point so interesting for meteorology elucidated by new researches.

4. The Society wishes an illustrated memoir containing very exact microscopic researches on the formation and development of the egg in the ovary of fishes and birds.

5. A monography as complete as possible of the lichens of the Netherlands.

6. We possess very exact determinations of the density and of the dilatation of mixtures of æthylic alcohol and water by Gilpin, Gay Lussac, and M. von Baumhauer. Determinations not less exact are requested for mixtures of water and methylic alcohol.

7. We only know with sufficient exactness the density of very few bodies soluble in water which might not be found by the ordinary hydraulic balance

Society requests the exact determination, at different temperatures, of at least fifty bodies soluble in water.

8. Of several plants, for instance, Aesculus hippocastanum, Amygdalus communis, Quercus pedunculata, Tilia parvifolia et grandifolia, Geranium, &c., a certain quantity of the ovules do not become developed. It is desirable that the cause of this constant anomaly should be explained by microscopic researches, illustrated by figures. These researches should comprise at least ten species of plants.

9. In volumetric researches the condensation of gases on surfaces exercises an embarrassing influence, inasmuch as the deficiency of precise data of such condensation at different temperatures and different pressures does not permit us to apply the necessary corrections. The Society solicits new researches on this important question.

10. Determinations of the temperature of deep stagnant waters (lacs) at different depths.

11. A minute description, based on new experiments, of fecundation in the family of the Gramineæ. An exact answer is sought to the following questions:

a. Do the anthers open before, after, or at the moment of the separation of the glumellæ (paleæ?)

b. Is the pollen strewn upon the stigmas before, after, or at the moment of the separation of the glumellæ ?

c. Does this separation of the glumellæ influence the descent of the pollen upon the stigmas?

d. What exterior causes may facilitate or prevent this descent of the pollen upon the stigmas ?

e. Do the stigmas secrete a matter qualified to retain the grains of pollen ?

f. By what route do the pollenary tubes descend towards the embryonic sack ?

These researches should comprise, in the first place, wheat, barley, rye, oats, and next as large a number as possible of other plants pertaining to different classes of the family of Gramineæ.

The Society would recall to mind that last year it proposed the following questions to be answered *before the first of January* 1866:

1. A complete embryology of the *Squalus spinax* and the *Squalus acanthias*, from the egg in the ovary to the complete formation of the young fish.

2. A critical nomenclature of the *Annulata* and *Turbellaria*, which are found in the interior and on the coasts of the Netherlands, based upon new researches.

3. A comparative myology of the anterior members of reptiles and of birds, with reference to the denomination of the corresponding or homologous muscles in mammifers, and especially in man.

4. The form of the figures, named after Lichtenberg, whether produced by positive or negative electricity, being different, a new and satisfactory explanation of that difference is requested.

5. Researches are invited on the molecular change produced in the wires of different metals by the sustained action of an electric current as strong as is possible without producing fusion.

6. A complete embryology of the *Lepas anatifera*.

7. A comparative anatomical description of the remains of birds which are found in different geological formations.

8. The origin of several rocks being still unknown to us, the Society wishes that at least one rock, at the choice of the author, should be examined with a view of deciding whether it has been deposited from a solution in water, or formed by the solidification of a mass melted by heat.

9. The preparation of solid carbonic acid presenting no longer any difficulty, or danger, a complete examination is needed of its physical properties.

10. A microscopic and chemical examination is requested of the matter, dif-

fusing a strong odor of musk, which is secreted by glands placed near the jaw-bones of crocodiles.

11. An exact anatomical description of the sturgeon, (*Accipenser sturio,*) with a monography of its development from the egg to the adult animal.

12. A comparison of the remains of castors and emydes found in the peat moss, in localities where those animals no longer live, with the living species of those animals.

13. If there be earthquakes which are only attributable to subsidence of strata situated at a greater or less depth, by what distinctive characters can they be recognized?

14. It has been observed that oxygen does not conduct the currents of induction of the apparatus of Ruhmkorff, except when its tension has been reduced to the pressure of 6 mm. of mercury; and that, from that point, its conducting power augments, when the diminution of tension is continued until it reaches 0.5 mm., when its powers appear to have attained a maximum. It is desirable that this phenomenon should be confirmed by new experiments, and that, by comparing it with what is analogous in regard to other gases, its cause should be made known.

15. New experimental researches on the velocity of propagation of electricity.

16. A methodical description of the remains of plants of the tertiary formation in the Netherlands.

17. Should metallic masses in a building protected by a lightning conductor be placed in communication with the latter or not? On this subject new researches, both experimental and theoretical, are requested.

18. A series of new experimental researches on the influence of mechanical forces upon chemical actions.

19. What are the constituent elements of the fatty body in the larvæ of insects, and what are its functions?

20. An anatomical description of the Monitor *(bivittatus)* of the island of Java.

21. The Society desires that the corpuscles of the blood in different orders of reptiles should be described and delineated after new and original researches.

22. The density of the vapors of substances is one of their most important physical characters; it is therefore to be regretted that it has as yet only been determined with respect to so few inorganic bodies. The method indicated by M. Sainte-Claire-Deville for that determination seems calculated to extend our knowledge upon this point. Hence the Society solicits the determination of the density of the vapors of different substances in regard to which that fact has remained undetermined till the present time.

23. Instruments and experimental methods having been sufficiently improved of late to enable us to undertake with success experiments on the diffraction of sound, the Society requests that those experiments be made, and that there be deduced from them a determination of the velocity of sound.

24. Researches are desired on the modifications in the spectral lines of divers substances, produced by different temperatures and densities.

25. An analytical catalogue *(raisonné)* is needed of the geographical distribution of mollusks.

The ordinary prize for a satisfactory answer to each of these questions is a gold medal of the value of 150 florins, and a further gratuity of 150 florins if the answer is deemed worthy thereof. The answers, legibly written in Dutch, French, English, Italian, Latin or German, (Roman letters,) and free of charge, with billets in the manner usually adopted in like cases, must be addressed to Professor E. H. VON BAUNHAUER, *Perpetual Secretary of the Holland Society of Sciences*, at Harlem.

IMPERIAL SOCIETY OF NATURAL SCIENCES OF CHERBOURG.

PROGRAMME OF COMPETITION FOR 1868.

The Society offers for competition the following question:

The varechs (sea-wrack, fucus) in the two-fold point of view of agriculture and of industry.

Full latitude is given to the competitors to treat this question as they shall think proper; yet it is thought that their attention should be more particularly called to the following points:

1. What time is the most suitable for gathering the varechs fixed on rocks, and best reconciles the requirements of reproduction with the wants and usages of agriculture? Should we make two harvests in each year, or but a single one? Ought the varech to be plucked or cut?

2. By what means is it practicable to conciliate to the greatest possible extent the interests of agriculturists and those of the manufacturers of iodine and of alkaline salts?

3. What are the most proper modes of application to augment the effects of manuring with the varechs? Might not the employment of a mixture in suitable proportions of the residuum of the lixiviation of kelp with the common varech be attended with good economical results? By what methods of previous stratification and maceration might we succeed in profitably uniting the mineral matters contained in that residuum with the organic substances indispensable for completing the elements of vegetable nutrition, and in obtaining the maximum of useful effect?

The answers to these different questions, and to all others which the competitors may themselves propose, should be founded not on theoretical considerations, but on new and solid experiments, devised by the authors of the memoirs, and carefully described. *The prize for competition is a gold medal of five hundred francs.* The Society may, moreover, award medals of silver to the authors of memoirs which, without completely resolving the proposed questions, shall have furnished, nevertheless, useful indications of a nature to merit that distinction. The right is reserved of publishing in the memoirs of the Society, either in whole or in part, the manuscripts which shall be presented.

The memoirs must be written in French, Latin, or English, and be addressed, free of charge, *before the first of July*, 1868, to the archivist of the Society, Dr. A. Le Jolis, rue de la Duchée, 29, at Cherbourg. Each manuscript will be preceded by a motto or epigraph, repeated in a sealed billet which contains the name and domicile of the author, as well as a declaration, signed by himself, that the production is unpublished, and has not before been submitted to competition. This billet will not be opened unless a prize shall have been awarded to the work, or it be judged worthy of publication in the memoirs of the Society. The manuscripts will remain the property of the Society.

Cherbourg, *June* 8, 1865.

MEMBERS OF THE BUREAU.

Secretary, L. Fleury. *Perpetual Archiviste*, Dr. A. Le Jolis.
Treasurer, Levieux. *Vice-President*, Geufroy, Senior.

PRIZE QUESTIONS OF THE PHYSICAL-MATHEMATICAL CLASS OF THE ROYAL PRUSSIAN ACADEMY OF SCIENCES FOR THE YEARS 1866–7.

PUBLISHED AT THE PUBLIC SESSION UPON THE LEIBNITZ FESTIVAL, JULY 7, 1864.

I.

(From the Steiner Legacy.)

In one of the monthly reports of the Academy, for January, 1856, as well as in an essay published in vol. LIII of Crell's Journal, Steiner communicated a series of the fundamental properties of the surfaces of the third order, arriving at the means for a purely geometrical theory of the same.

The Academy desires that this remarkable labor of the distinguished geometer be carried out further, according to its synthetic methods, and be perfected in several essential points.

For this purpose it would be necessary, first, to give proofs, which are for the most part merely indicated or even omitted for the principal propositions; but the investigation must be extended beyond the cases treated by Steiner to those surfaces in which the elements serving for the geometrical construction are in part imaginary. Beside this, the Academy would regard it as an important perfection of Steiner's theory, although not absolutely indispensable, if a complete characterizing of the different kinds of cones in space formed by the intersection of two of the surfaces here alluded to were given.

Essays contending for the prize may be written in the German, Latin, or French languages, and the time during which they can be received will expire upon March 1, 1866. Every essay must be accompanied by a motto, which must also be placed upon the outside of a sealed envelope containing the author's name.

The prize of six hundred thalers will be conferred at the public session of the Leibnitz festival in the month of July, 1866.

II.

The theory of the elliptic and Abel's functions, which has already enabled the solution of problems in nearly every branch of mathematics, and for which former means for the analysis were insufficient, is without doubt susceptible of numerous additional applications, and, therefore, the Academy offers the following prize questions:

"For any important problem in algebra, arithmetic, integral calculus, geometry, mechanics, and mathematical physics, which can be perfectly solved by aid of the elliptic or Abel's transcendentals."

The essays may, at the will of the author, be written in the German, Latin, or French languages, and the time of their reception expires upon the 1st of March, 1867. Every essay must be accompanied with a motto, which must also be written upon the outside of a sealed envelope containing the author's name.

The prize of one hundred ducats will be conferred at the public session on the Leibnitz festival in July, 1867.

GEOLOGICAL PRIZE.

ANNOUNCED ON MAY 30, 1864, BY THE IMPERIAL ACADEMY OF SCIENCES AT VIENNA.

The great majority of the most accurately studied eruptive rocks, both in and outside of Austria, belong either to the older palæozoic formations or to the later tertiary and quite modern periods.

In the Austrian alps, however, still more in the Carpathian mountains, and also partially in Bohemia, there are masses of rocks in great quantity and variety, which break through the stratified rocks or stand in relation to the same, but of which the period of eruption falls within the epoch of a middle age, beginning with the dyas formation to that of the eocene.

To name only a few of these, we have the metaphyre of the ——— (Rothliegende) in Bohemia and the red sandstones of the Carpathians, which belong probably to the same formation; the red porphyry and metaphyre of the trias of the southern Alps; the so-called augite-porphyry and amygdaloids of the east Carpathians standing in connexion with the jurassic limestone; the teschinite of the chalk and eocene formation of the Silesian Carpathians, &c.

Many of these rocks have been named heretofore generally and from mere external analogies. An accurate mineralogical and chemical investigation of the same, a comparison of the platonic rocks of the higher and younger periods, constitutes a problem, the solution of which would fill a gap, in the true sense of the word, in our knowledge, and would appear alone to be of vast importance to science. The Imperial Academy can with right expect this solution, because, as far as is yet known, no other country in the world possesses eruptive rocks of the period alluded to in equal quantity and abundance.

The mathematical-natural history class of the Imperial Academy of Sciences has therefore determined to offer a prize for the answer to the following problem, viz:

"*An accurate mineralogical, and, as far as may be possible, chemical investigation of the largest number of eruptive rocks in Austria of the middle period, from the dyas formation to that of the eocene, and the comparison of these with the more accurately known older and younger eruptive rocks of Austria and of other countries.*"

The period for closing the reception of the prize essays has been fixed for December 31, 1866; the announcement and reception of the prize of two hundred royal imperial mint ducats will follow at the commemorative session of the Academy on May 30, 1867.

The following paragraphs relating to prize essays, from the order of business of the Imperial Academy, are published for the benefit of the contestants:

§ 56. All prize essays should be furnished without the name of the author, but, as usual, with an accompanying motto and with a sealed envelope containing the author's name in the inside and his motto upon the outside.

At the commemorative session of May 30, the president shall open the sealed envelope inscribed with the motto of the successful essay, and shall announce the name of the author. The other envelopes shall be burned unopened, but the essays shall be preserved subject to the call of their authors, announcing each his motto.

§ 57. The division of a prize between several contestants is prohibited.

§ 58. Every crowned prize essay remains the property of its composer. Should he so desire, it will be published by the Academy.

§ 59. The members of the Academy shall not strive for this prize.

§ 60. Essays which have not received a prize may, if worthy, be published by the Academy, with the consent of the author.

EXPLORATIONS.

SCIENTIFIC EXPEDITION TO MEXICO.

A REPORT ADDRESSED TO THE EMPEROR BY THE MINISTER OF PUBLIC INSTRUCTION.

TRANSLATED FOR THE SMITHSONIAN INSTITUTION.

SIXTY-SIX years ago 40,000 men of the army of Italy and our most illustrious chief landed at Alexandria. The young general was followed not only by the bravest soldiers in the world, but by a whole colony of savants, who achieved, after their own manner, the conquest of Egypt by tearing away the veil in which its ancient civilization had, for fifteen centuries, been enveloped. By the researches of the *Institute of Cairo* the archæological sciences were renovated in Europe. Without the publication of the great work of the *Description of Egypt,* Champollion would never have conceived the design nor possessed the means of commencing the interpretation of the hieroglyphs which science had pronounced an inexplicable enigma, and France would have wanted the honor of having found the key to those inscriptions which have already explained so many symbols and doctrines, and revealed so many ideas concerning the religion, the history, and the chronology of that ancient world.

It was on the banks of the Nile that Geoffroy Saint Hilaire conceived the first thought of his great system of anatomical philosophy; and if the levellings effected by his colleagues on the Isthmus of Suez, under the fire of the Arabs, were not exact, their idea of a communication between the two seas has not the less maintained its popularity to the day when, thanks to another Frenchman, it might become a reality.

To the conquests of abstract science were added those of art. In the drawings which the expedition brought away our artists saw enlarged resources for the expression of the beautiful placed at their disposal.

The labors of the Institute of Cairo were even attended with consequences of practical utility. The study of the climate and of the geographical conditions of the valley of the Nile led to the discovery of means for promoting the salubrity of the country and securing to its inhabitants a better hygiene. At this day the plague has almost disappeared, and, notwithstanding the frequency and facility of commercial intercourse, this scourge no longer arrives on our coasts to decimate our population, and, as in 1720, to snatch from Provence alone 85,000 of its inhabitants. It is to the medical investigations of the Institute of Egypt that we should refer the commencement of this great amelioration.

And while learned Europe was enriched with scientific facts, with ideas and forms of art, which the great work on Egypt threw into general circulation, Egypt itself, reanimated by the contact with our soldiers and savants, emerged from its lethargy. Several of its youth were consigned to a member of the Institute to be initiated in our European civilization; a number of our engineers were invited to the work of Egyptian regeneration; and if there is found to-day on the

banks of the Nile an association which asserts its place among modern societies, and plays an important part, through production and commerce, in the general interests of the world, it is in great part because the hand of France was stretched forth to rescue this people from their state of torpor.

Prepossessed by these memories, your Majesty has determined that what was done on the banks of the Nile by him who was to become Napoleon I, should be accomplished in Mexico under the auspices of Napoleon III. The results obtained sixty years ago are the guarantee of the results in reserve for the new expedition. Mexico, it is true, does not offer the historic interest presented by that land of Egypt, where Herodotus placed the origin of the religion, the arts, and of a portion of the inhabitants of Greece. Nevertheless Mexico, too, has many secrets to disclose to us: a peculiar civilization which science should revive, races whose origin eludes us, unknown languages, mysterious inscriptions, and imposing monuments. But if the expedition be contemplated in relation to the natural and physical sciences, what comparison can be made between the two countries? On the one hand, a long valley of scarcely 260 leagues, with a width at certain points of but a few hundred toises, where the sky, the earth, and the waters are of an admirable but wearisome uniformity; on the other, a vast region bathed by two oceans, traversed by large rivers and lofty mountains—which, situated near the equator, possesses every climate, because it has all altitudes; where the redundant vegetation of the tropics shelters innumerable tribes of animated creatures; where, in fine, the internal wealth corresponds to that of the surface, for the millions which, during three centuries, Mexico has poured into the lap of Europe are but the first fruits of the treasures which it yet has in store.

The Mexico of Montezuma comprised but about six degrees of latitude, from the 15th to the 21st. Outside of its frontiers there remained, to the south, Yucatan and the entire isthmus; to the north, all Sonora and the great valley of the Rio del Norte. But the history of these regions, the races which people them, is too closely associated with the history and the races of Mexico to be neglected by a scientific expedition. The field of exploration, then, extends from the sources of the Rio del Norte and the river Colorado to the Gulf of Darien, over about 32° of latitude.

It is true that a great number of documents relative to this wide tract have been already collected by the scientific men of the country—by some of the functionaries sent thither by France, and by travellers, who have followed in the traces of the most illustrious of their number, William von Humboldt. But information thus gained at points widely separated requires to be compared, digested, and submitted to scientific verification. In view of the details and rigor of method which science now exacts, Mexico offers, in regard to many sciences, a field of culture almost untouched. We have, for example, numerous charts of this region, but the best of them leave much to be desired. In the provinces to the south and west of Mexico the course of the largest rivers is traced in a very uncertain manner, and it is not necessary to diverge far from the frequented routes to make the most unexpected discoveries. At a short distance from Perote, on the highway between Vera Cruz and Mexico, the maps indicated, four or five years since, a lagoon, where M. de Saussure encountered hills. To the north the region of the Sierra Madre and the Sierra Verde, to the south Guatemala, Honduras, and Darien, include vast tracts as little known as the centre of Africa.

These researches, useful alike to commerce and to science, will promote, perhaps, the solution of the problem propounded twenty years ago by Prince Louis Napoleon for piercing the American isthmus with an interoceanic canal. The Emperor might in this, as in so many other instances, witness the realization of the hardy and prolific visions of the exile.

In regard to the geologic constitution of this part of the New World we have

glimpses rather than any general view; two studies, among others, are here to be created or resumed—paleontology and the examination of volcanic phenomena, which, in Mexico, present themselves in colossal proportions.

What has been done by mining associations is worthy of praise, but what is known as yet of the mineralogical wealth of Mexico is undoubtedly the smallest part of that which exists. The sites thus far turned to profit are those for the most part which hazard has presented. A truly scientific exploration would disclose to this branch of industry a future of unexpected prosperity.

A country of so bold and broken an outline, and submitted to the powerful and varied action of extremes of climate as well as to that of the forces proceeding from the interior of the earth, holds out large promise to the physics of the globe and to meteorology.

Nor will botany be less fortunate. Those endlessly diversified formations, those elevations where all climates present themselves, enable Mexico and Central America to spread before the eyes of the traveller a magnificent and multiform flora, such as is offered by no other region of the globe. The science has here already made many acquisitions, but a full harvest is yet to be gathered, and our gardens, our parks, our forests, and our fields will be enriched with new plants for ornament and use.

In these regions where nature scatters life under all forms, the animal kingdom is not less rich nor less curious than the vegetable. Agassiz thinks that he has found living in the Gulf of Mexico certain antediluvian polyps which occur imbedded in the soil of Florida, and the *encrinites* are extant only here: a mysterious link between the world of primeval times and our own.

The almost new science of anthropology cannot fail to derive great light from the calm study of the races buried in the grottoes of Central America, or from those which still live on the plateau of Anahuac or in the neighboring regions. The mixed breeds, resulting from the intercourse of the indigenous and foreign races, will furnish suggestions for a study associated with questions of the highest import, at once physiological, moral, and social.

Mexico is still rich in promise for another science—comparative philology. Though hardly of a man's age, this science has already detected the origin of races of men, reunited the broken ties of nations, and prepared the solution of the great problem of the variety or unity of our species; a question which seemed to possess no interest but for scientific curiosity, yet one which, for three years, North America has been seeking to decide in the furious conflicts of a more than civil war. The study of Mexican idioms was commenced, it is true, long since, but there is room for pursuing it on a wider scale. The ruins of Palenque cover mysteries comparable, perhaps, to those which the expedition of Egypt found on the banks of the Nile, and which, thanks to that expedition, Champollion was finally enabled to penetrate.

The Mexico of Montezuma has almost wholly disappeared; the expedition will afford the means of its rediscovery. Perhaps our explorers will bring to light some of those rare Mexican manuscripts or *yukatéques* which have escaped recurring devastations. They will certainly collect those oral traditions of which modern criticism so well knows how to avail itself.

In 1855, M. de Saussure discovered, within a few leagues of Perote, an entire city of which, before him, no one had any knowledge. An American traveller, passing directly from the sea to Mexico, by a route traced by himself, encountered from eighteen to twenty considerable monuments, of which the memory was lost. The Mexican solitudes have similar surprises in reserve for our savants. It will be theirs to renovate this great and curious page of the world's annals, which centuries have effaced, and our generation, so arid of the noble emotions of history, will see a wider horizon opened for its contemplation.

When our soldiers quit this country, leaving glorious memories behind them, our savants will complete the conquest of it for science. There is no room to

doubt but that, by virtue of their labors, certain branches of our knowledge will be stimulated and extended—others, perhaps, created, and that new facts will produce new and fruitful ideas which shall give to our comprehensive studies a salutary impulse.

In order to assure to the scientific expedition of Mexico every guarantee of success, I have the honor to propose that your Majesty will be pleased to constitute, by decree, a commission which shall hold its sessions at the department of public instruction. Composed of men eminent in science and in the state, or of savants who have already explored Central America, this commission will give to travellers the necessary instructions, will follow the progress of the expedition, and will prepare for the scientific world the publication of a work which will prove, I trust, a monument of the patronage which your Majesty condescends so directly to accord to this noble enterprise.

I flatter myself that the public offices will cordially associate themselves with the designs of the Emperor; and, consequently, I have also the honor of praying your Majesty to cause to be laid for examination before the council of state the draught of a law for opening a credit of 200,000 francs in behalf of the ministry of public instruction in order to defray the expenses of the expedition.

A JOURNEY TO THE YOUCAN, RUSSIAN AMERICA.

BY W. W. KIRBY.

I left home on the 2d of May in a canoe paddled by a couple of Indians belonging to my mission. We followed the ice down the noble McKenzie, staying awhile with Indians wherever we met them, and remained three or four days at each of the forts along the route. On the 11th of June I left the zone in which my life had hitherto been passed, and entered the less genial *arctic* one. Then, however, it was pleasant enough. The immense masses of ice piled on each side of the river sufficiently cooled the atmosphere to make the travelling enjoyable, while the sun shed upon us the comfort of light nearly the whole twenty-four hours. And as we advanced further northward he did not leave us at all. Frequently did I see him describe a complete circle in the heavens.

Betwen Point Separation and Peel's river we met several parties of Esquimaux, all of whom, from their thievish propensities, gave us a great deal of trouble, and very glad were we to escape out of their hands without loss or injury. They are a fine-looking race of people, and from their general habits and appearance, I imagine them to be much more intelligent than the Indians. And if proof were wanting I think we have it in a girl who was brought up from the coast little more than three years ago, and who now speaks and reads the English language with considerable accuracy. The men are tall, active, and remarkably strong, many of them having a profusion of whiskers and beard. The women are rather short, but comparatively fair, and possess very regular and by no means badly formed features. The females have a very singular practice of periodically cutting the hair from the crown of their husband's head, (leaving a bare place like the tonsure of a Roman Catholic priest,) and fastening the spoil to their own, wear it in bunches on each side of their face, and a third on the top of their head, something in the manner of the Japanese who recently visited the United States. This custom, as you will imagine, by no means improved either their figure or appearance, and as they advance in life, the bundles must become to them uncomfortably large. A very benevolent old lady was most urgent for me to partake of a slice of blubber, but I need hardly say that a sense of *taste* caused me firmly but respectfully to decline accepting her hospitality. Both sexes are inveterate smokers. Their pipes they manufacture themselves, and are made principally of copper; in shape, the bowl is very like a reel used for cotton, and the hole through the centre of it is as large as the aperture of the pipe for holding the tobacco. This they fill, and when lighted will not allow a single whiff to escape, but in the most unsmoker-like manner swallow it all, withholding respiration until the pipe is finished. The effect of this upon their nervous system is extremely great, and often do they fall on the ground completely exhausted, and for a few minutes tremble like an aspen leaf. The heavy beards of the men, and the fair complexions of all, astonished my Indians greatly, and in their surprise called them "Manooli Conde," like white people. They were all exceedingly well dressed in deer-skin clothing, with the hair outside, which being new and nicely ornamented with white fur, gave them a clean and very comfortable appearance. Their little Kyachs were beautifully made, and all the men were well armed with deadly-looking knives, spears, and arrows, all of their own manufacture. The Indians are much afraid of them, and so afraid of my safety were two different parties that I saw on my

way down that a man from each of them, who could speak a little Eskimos volunteered to accompany me, without fee or reward, and invaluable did I find their services. Poor fellows! they will never see this; but I cannot refrain from paying them here my tribute of gratitude and thanks.

At Peel's river I met with a large number of Loucheux Indians, all of whom received me most kindly, and listened attentively to the glad tidings of salvation I brought unto them. As these are a part of the great family who reach to the Youcan and beyond, I need not dwell upon them here, as their habits will be included in a general description that I shall give of the whole by-and-by. I may, however, remark that from their longer association with the whites many of the darker traits that belong to their brethren on the Youcan apply, if at all, in a much milder form to the Indians there and at Lapiene's House.

I left my canoe and Indians, as well as those who accompanied me, at the fort, and taking two others who knew the way, walked over the Rocky mountains to Lapiene's House. This part of the journey fatigued me exceedingly—not so much from the distance (which was only from 75 to 100 miles) as from the badness of the walking, intense heat of the sun, and myriads of the most voracious mosquitos that I have met with in the country. The former, I think, would justly defy competition. There were several rivers to ford, which from the melting snows and recent rains were just at their height. Fortunately they were neither very deep nor wide, or my size and strength would have been serious impediments to my getting over them.

At Lapiene's House I was delighted to meet Mr. Jones, who was my companion on travel from Red river to Fort Simpson. He had come up in charge of the Youcan boat, and at once kindly granted me a passage down with him. I had fortunately a bundle of Canadian newspapers in my carpet-bag, some of them containing some speeches on educational subjects by his venerable grandfather, the bishop of Toronto. Five days of drifting and rowing down the rapid current of the Porcupine river brought us to its confluence with the Youcan, on the banks of which, about three miles above the junction, the fort is placed. My friend Mr. Lockhart was in charge, and all who know the kindness of his heart need not to be told of the cordial reception that I met with from him. Another hearty grasp was from the energetic naturalist Mr. R. Kennicott, who, under the auspices of the Smithsonian Institution, came into the district with me, and passed the greater part of his first winter at Fort Simpson. He delighted me with the assurance that he had met with a vast field, and that his efforts had been crowned with much success, especially in the collection of eggs, many rare and some hitherto unknown ones having been obtained by him; so that the cause of science, in that department, will be greatly benefited by his labors. Among many others I noticed the eggs and parent birds of the American widgeon, the black duck, canvas-back duck, spirit duck, (*Bucephala abeola,*) small black-head duck, (*Fulix affinis,*) the waxwing, (*Ampelis garrulus,*) the Kentucky warbler, the trumpeter swan, the duck-hawk, (*Falco anatum,*) and two species of juncos. With the exception of the waxwing, however, there were few that have not been obtained in other parts of the district by the persevering zeal of Mr. Ross, the gentleman in charge, and it, I have since learned, nested numerously in the vicinity of my out-station at Bear lake.

On my arrival at the Youcan there were about 500 Indians present, all of whom were astonished, but appeared glad, to see a missionary among them. They are naturally a fierce, turbulent, and cruel race, approximating more nearly to the Plain tribes than to the quiet Chipewyans of the McKenzie valley. They commence somewhere about the 65th degree of north latitude, and stretch westward from the McKenzie to Behring's straits. They were formerly very numerous, but wars among themselves and with the Esquimaux have sadly diminished them. They are, however, still a strong and powerful people. They are divided into many petty tribes, each having its own chief, as the Tā-tlit-Kutchin. (Peel's

River Indians,) Tā-Kūth-Kutchin, (Lapiene's House Indians,) Kutch-a-Kutchin. (Youcan Indians,) Touchon-ta-Kutchin, (Wooded Country Indians,) and many others. But the general appearance, dress, customs, and habits of all are pretty much the same, and all go under the general names of Kutchin (the people) and Loucheux, (squinters) The former is their own appellation, while the latter was given to them by the whites. There is, however, another division among them of a more interesting and important character than that of the tribes just mentioned. Irrespective of tribe, they are divided into three classes, termed, respectively, Chit-sa, Nate-sa, and Tanges-at-sa—faintly representing the aristocracy, the middle classes, and the poorer orders of civilized nations, the former being the most wealthy and the latter the poorest. In one respect, however, they greatly differ, it being the rule for a man not to marry in his own, but to take a wife from either of the other classes. A Chit-sa gentleman will marry a Tanges-at-sa peasant without the least feeling *infra dig*. The offspring in every case belong to the class of the mother. This arrangement has had a most beneficial effect in allaying the deadly feuds formerly so frequent among them. I witnessed one this summer, but it was far from being of a disastrous nature. The weapons used were neither the native bow nor imported gun, but the unruly tongue, and even it was used in the least objectionable way. A chief, whose tribe was in disgrace for a murder committed the summer before, met the chief of the tribe to which the victim belonged, and in the presence of all commenced a brilliant oration in favor of him and his people, while he feelingly deplored his own and his people's inferiority. At once, in the most gallant way, the offended chief, in a speech equally warm, refuted the compliments so freely offered, and returned them all, with interest, upon his antagonist. This lasted for an hour or two, when the offender, by a skilful piece of tactics, confessed himself so thoroughly beaten that he should never be able to open his lips again in the presence of his generous conqueror. Harmony, of course, was the inevitable result.

The dress of all is pretty much the same. It consists of a tunic or shirt reaching to the knees, and very much ornamented with beads, and Hyaqua shells from the Columbia. The trousers and shoes are attached, and ornamented with beads and shells similar to the tunics. The dress of the women is the same as that of the men, with the exception of the tunic being round instead of pointed in front.

The beads above mentioned constitute the Indian's wealth. They are strung up in lengths, in yards and fathoms, and form a regular currency among them, a fathom being the standard, and equivalent to the "made beaver" of the company. Some tribes, especially the Kutch-a-Kutchin, are essentially traders, and, instead of hunting themselves, they purchase their furs from distant tribes, among whom they regularly make excursions. Often the medicine-men and chiefs have more beads than they can carry abroad with them, and when this happens the company's stores are converted into banking establishments, where the deposits are invested for safe keeping. The women are much fewer in number and live a much shorter time than the men. The latter arises from their early marriages, harsh treatment they receive, and laborious work they have daily to perform, while the former is caused, I fear, by the cruel acts of infanticide which to female children have been so sadly prevalent among them. Praiseworthy efforts have been made by the company's officers to prevent it, but the anguished and hardened mothers have replied that they did it to prevent the child from experiencing the hardships they endured.

The men much reminded me of Plain tribes, with their "birds and feathers, nose jewels of tin, and necklaces of brass," and plentiful supply of paint, which was almost the first time I had seen it used in the district. Instead of the nose jewels being of "tin" they were composed of the Hyaqua shells which gave the expression of the face a singular appearance. The women did not use

much paint, its absence was atoned for by tatooing, which appeared universal among them. This singular custom seems to be one of the most widely diffused practices of savage life; and was not unknown among the ancients, as it, or something like it, seems to be forbidden to the Jews, "ye shall not print any marks upon you," Lev. xix, 28.

Polygamy, as in almost all other barbarous nations, is very prevalent among them, and is often the source of much domestic unhappiness among them. The New Zealander multiplies his wives for show, but the object of the Kutchin is to have a greater number of poor creatures whom he can use as beasts of burden for hauling his wood, carrying his meat, and performing the drudgery of his camp. They marry young, but no courtship precedes, nor does any ceremony attend the union. All that is requisite is the sanction of the mother of the girl, and often is it a matter of negotiation between her and the suitor when the girl is in her childhood. The father has no voice in the matter whatever, nor any other of the girl's relatives.

The tribes frequenting Peel's river bury their dead on stages, the corpse being securely enclosed in a rude coffin made of hollowed trees. About the Youcan they were formerly burnt, the ashes collected, placed in a bag, and suspended on the top of a painted pole. 'Nightly wailings follow for a time, when the nearest relative makes a feast, invites his friends, and for a week or so the dead dance is performed, and a funeral dirge sung, after which all grief for the deceased is ended. I witnessed their dance at the fort, and have been told by others that the dead song is full of wild and plainitive strains, far superior to the music of any other tribes in the country.

Altars, or rites of religion, they had none, and before the traders went there not even an idea of a God to be worshipped. Medicine men they had, in whose powers they placed implicit faith; and whose aid they dearly purchased in seasons of sickness or distress. They were, emphatically, a people 'without God in the world." Knowing their prejudices, I commenced my labors among them with much fear and trembling; but earnestly looking to God for help and strength, and cannot doubt that both were granted. For, before I left, the medicine men openly renounced their craft, polygamists freely offered to give up their wives, murderers confessed their crimes, and mothers told of deeds of infanticide that sickened one to hear. Then all earnestly sought for pardon and grace. Oh! it was a goodly sight to see that vast number, on bended knees, worshipping the God of their salvation, and learning daily to syllable the name of Jesus. Since my return I have read a glowing picture of savage life, when left to its native woods and streams, and heartily as I feel that I could be a friend of him who is, in truth, the friend of the aborigines, yet sadly do I feel that between theory and fact there is often a gaping discrepancy. To draw a picture of savage life is one thing, to see "the heathen in his darkness" is another. To speak of the Indian roaming through his native woods, now skimming over the glassy lake, or floating down the silent current, may be to show the poetry of his life; but there is the sterner chapter of reality to place over against it. From that chapter the above remarks have been gathered, they present the heathen as they are in themselves. For twenty years have not yet elapsed since the white man planted his foot in the Youcan valley, and since he has been there his influence has been to improve, and not to contaminate. And if a testimony be valuable, more from the cause to which it is given than from the source whence it proceeds, most heartily do I bear mine to the humane and considerate treatment that the Indians of the Mackenzie river district receive from the officers of the company. In many instances that I could mention, the officer is more like the parent of a large family of adult children than what his position represents. The undoubted fact is, that the whole tendency of heathenism is to brutalize and debase, while it remains with civilization and the Gospel to elevate and to bless.

Should you desire, I shall be happy next season to give you a few of the Indian legends, as well as some account of the geology and fauna of my journey. The flora, I do not sufficiently understand to say anything about, although, from the great variety of plants that I saw, there must have been many interesting to botanists. When at Red river, I read a paper by Mr. Barnston, on the growth of the onion on the banks of the Porcupine river, and I have much pleasure in being able to confirm his statements, that it is not the real onion, but the chive that grows in such abundance there.

EXPLORATION IN UPPER CALIFORNIA IN 1860,

UNDER

THE AUSPICES OF THE SMITHSONIAN INSTITUTION.

BY JOHN FEILNER, U. S. A.

HAVING been requested by the Smithsonian Institution to make collections of birds, eggs, nests, &c., in the vicinity of Rhett and Klamath lakes, in the northern part of this State, I applied to Captain John Adams, 1st United States dragoons, commanding Fort Crook, California, for permission to visit the lakes, and was granted a furlough of twenty days for this purpose, and was also furnished with two pack animals to transport whatever collections I might secure. Captain Adams not deeming it prudent for me to venture among the Indians entirely alone, and wishing to render all assistance in his power for the prosecution of the object in view, allowed private Alexander Guise, company F, 1st dragoons, to accompany me. The following is the itinerary of our journey:

May 13.—Left this post *en route* for Klamath lake, via Yreka; camped at Bear creek; distance, 20 miles.

May 14.—After re-arranging our traps, continued our way along the Yreka road, which we found exceedingly rough. Very few birds have at this early part of the season made their appearance; for, although a spring month, everything wears an aspect of winter, and travelling through a thickly timbered country and over rocky roads, one has not the same chance for observation that a clear and more level country would afford. Of the birds seen to-day I observed *Picus albolarvatus*, *Picus harrisii*, *Sitta aculeata*, *Sitta canadensis*, and *Junco oregonus;* but *Spyrapicus williamsonii*, which heretofore I have found abundant in this section of country, seemed to have wholly disappeared. I did not see even one specimen. At the station-house on McCloud river I saw for the first time, in large numbers, the species marked No. 511, of which I collected several, but lost all, save one, when attacked by Indians. Camped at Pilgrim's camp, on northwest branch of McCloud river; distance 30 miles. After having left the station-house about two or three miles behind, we suddenly struck a desert of about six miles in extent, entirely of sand, and not a particle of snow to be seen. This sudden change from deep snow to a barren sand level, from cold to heat, was very surprising. The desert is between station-house on McCloud river and Pilgrim's camp on the same stream, and although a desert itself, has the appearance of an oasis in a snow desert.

May 15.—Left Pilgrim's camp early, deeming it more advisable to travel for warmth than to remain shivering in camp. The winter in this section of the country being very severe, the springs are necessarily backward, and one will often find snow on this road in June; in fact, "Shasta butte," in our immediate vicinity, is covered with snow the whole year, presenting a very picturesque appearance. The height of the mountain is very great, and it can be seen a long distance off. To-day, saw large numbers of the Canada jay, (*Perisoreus canadensis.*) I collected several, but could save one only, (No. 508.) *Gymnokitta cyanocephala*, not noticed by me at Fort Crook, California, for the past year, were seen in numbers to-day. The country passed over was, with

very few exceptions, a barren waste. The soil does not admit of cultivation to a sufficient extent to repay the laborer, with the exception of a few fertile spots on the road, station-house being one; but the intense cold of a single winter would induce almost any one to seek a more genial region. The mercury at Pilgrim's camp ranges from 19° to 25°, and snow falls to the depth of *fifteen feet*. Distance travelled, 25 miles; camp, Sheep Rock.

May 16.—From Sheep Rock to Yreka the country presents a very unprepossessing appearance, and is almost entirely destitute of birds. The great shrike (*Collyrio borealis*) might be said to be numerous about the cedar bushes near Sheep Rock. The only thing of interest I observed was the myriads of locusts which swarm this section of country, destroying every green thing, even to the foliage of the tallest trees. The mountain tops appear alone to have escaped this plague. Their encroachment on the gardens of Yreka compelled the owners to find some means of protecting their produce, and I believe the following described method has proved successful:* A narrow board, six inches wide, is placed on the ground on its edge; another narrow board, having one edge guarded by a strip of tin tacked on so as to project slightly beyond the edge, is nailed on at right angles to the first board, forming, as it were, a rectangular gutter. A succession of these gutters fastened together forms the "locust" or "cricket" fence. The smooth surface presented by the tin causes the cricket to drop off, and after many fruitless efforts to "scale the fence," they abandon the project. I have spoken at length of the habits of the crickets in my appended "list of birds, &c." Arrived at Yreka early in the afternoon; distance, 20 miles. Yreka is one of the principal mining towns of California. Having a letter of introduction to Judge Rosenborough, formerly Indian agent, I presented myself to him, hoping that through his assistance I might procure the services of some Indian guides. Judge R. endeavored to dissuade me from continuing my tour, and even warned me against the probable hostility of the Indians on Klamath river; but thinking that probably I might purchase peace by distributing a few trinkets among the Indians, I provided myself with such articles as I thought they would fancy, and determined to make the attempt at all events. Therefore, after remaining in Yreka one day, I was obliged to start for the lakes without guides, and accompanied by Guise only.

May 18.—Started for Bogus mountains; travelled all day; camping at night on Big Bogus creek; distance, 20 miles; collected largely of birds and nests; but as the species of the former differ from those already collected by me on Fall river, their names are unknown. The country around here is well adapted to agricultural purposes, and a slight labor would insure large produce.

May 19.—After striking the Klamath river, I travelled on it for eight miles until I came to Hot Springs, where we encamped. I learned, afterwards, from the Indians, that these springs are held in high estimation on account of their medicinal properties. The springs are on both sides of the river, some of them so close to it that a person can stand on the bank and put a hand in each at the same time. It was within one mile of these springs that I first had intimation of the hostility of the Indians, who, as soon as they saw us, made off to the mountains. After having been in camp some few minutes, I saw a smoke at no great distance from us, and upon approaching to ascertain its cause, found a rancheria which had but recently been abandoned and set on fire. This act, indicative of hostile intentions on the part of the Indians, caused me to change my course of travel.

May 20.—Made a detour to northeast, crossing some very high and steep mountains; found the "dusky grouse" quite numerous, but could not find any nests. About noon, arriving in sight of Butte Valley lake, I met a tribe of Indians, headed by their chief, "Ike." We had a long talk together, the result of which was not

* This method is more fully described with woodcuts in the Smithsonian report for 1860.

at all favorable to the further continuance of my trip. "Ike" was exceedingly inquisitive, and was as thorough in his surveillance as would have been a custom-house officer; he declined affording me any assistance, and by pointing in the direction whence we came, intimated that discretion on our side would be the better part of valor. Accordingly, I directed my course towards the south side of the valley, and came across some white cattle-herders. I endeavored to find Butte creek, marked on Lieutenant Williamson's map, but could not, though I should have been in its immediate vicinity. I was informed by the herders and some friendly Lil-lac Indians that no creek or river leaves the valley, the stream forming a lake and sinking. Distance to-day 20 miles.

The country passed over to-day was well watered and timbered, and indicated fine agricultural capabilities. As the tide of emigration flows to this section of the country, so will it rise in wealth and importance, and the only drawback at present is the presence of the hostile Indians. During a conversation held with the herders above mentioned, I learned that the Indians had recently killed several head of cattle, and had manifested a desire to annoy the whites to such an extent that they would be obliged to leave the country. The Indians are very jealous, and consider every emigration of whites into their country as an encroachment upon their rights.

May 21.—To-day I was quite successful in collecting specimens, and as the fruit of my labor I can enumerate several varieties of water-fowl eggs, nests, &c., but I regret that our engagement with the Indians (spoken of hereafter) deprived me of most of them. The "brown curlew" (*Numenius longirostris*) was very abundant here; but from the several collected, I have been able to preserve but one nest, containing three eggs. When found, all the nests contained four eggs, and as nicely arranged as if placed by hand. Large numbers of "mud hens," or coots, (*Fulica americana,*) were seen; in fact, they were the most abundant bird breeding; they lay from nine to eleven eggs; some of them I saved; they build their nests of tules, and select the edge of the tule course, and by breaking them down and building on them, their nests have the appearance of floating baskets. The friendly Indians, who are living with the herders, promised to assist me on the following day, and with their aid I anticipated great success.

May 22.—The three Indians who yesterday promised me their assistance disappointed me, and assigned as a reason for refusing to accompany me, that the Indians of Ike's band having recently stolen cattle from the whites, they feared a general fight. The herders also said that the Indians had stolen several head of cattle, and that Ike's band were the perpetrators of the theft, and asked me if I would accompany them in visiting the chief and talk with him; if they could exact a promise of good faith on his part for the future, they were willing to overlook the past, and also make him some presents of cattle. Accordingly we went to Ike's camp, stated our object, and expressed a desire to live at peace with him, and endeavored to impress upon him the idea that he had acted wrongly in stealing cattle; that such acts would exasperate the white men, who would come in great numbers and kill his whole tribe. Ike listened very stoically, and replied that he had killed both men and cattle, and it was his intention to do so again; he said the country belonged to the Indians; that the whites had no business there, and that he would drive them off. Finding that we could not come to pacific terms with Ike, we determined to return to the herders' camp, there to consult as to the course to be pursued, and I to prepare for the continuance of my trip; but we had not gone more than three hundred yards when the Indians fired on us; they numbered thirty or forty, we but four, including one friendly Indian; we were obliged, therefore, to seek shelter in the timber; but so hotly did they pursue us that we had but time to make good our retreat to a deserted cabin some six miles distant, which we barricaded as best we could, and made "loop holes," through which to discharge our pieces. Thus

fortified, we could have withstood the assault of quite a large force; but one of our party, a young married man, had a wife in a cabin some two miles distant, and as he was apprehensive of her safety, he determined to go to her, which he succeeded in doing, escaping the vigilance of the Indians. Our cabin was attacked several times, but we beat off our assailants and took advantage of the cover of night to make good our retreat to the cabin of the married man, where I found my companion Guise, who had preceded me.

May 23.—To-day the three friendly Indians left us, but were forced to return, as they were attacked by Ike's band. A re-enforcement of three white men having arrived, we determined to leave the cabin and give the Indians battle, which we did, and succeeded in driving them to the mountains, with a loss on their part of their chief and several warriors. We captured some eighteen horses, two fine rifles, saddles, &c. Our party escaped unharmed, which was somewhat remarkable, as the Indians largely outnumbered us, and were good marksmen.

Learning from one of the men who just joined us that nearly all the troops at Fort Crook had been ordered to Carson valley, on account of Indian outbreaks, I was desirous of reaching home as soon as possible; and as soon as I could pack what few specimens the Indians had not destroyed, and make some necessary preparation for the road, I started, and arrived at Fort Crook, California, on the 27th instant. My collections were subsequently forwarded to the Smithsonian Institution.

From observation and information received from the Indians, I am convinced that a very interesting collection of nests and eggs of water-birds could be made from the Rhett and Klamath lakes, May and June being the most favorable months for collecting. May, and first week or so of June, for water-fowls; latter part of June for land-birds. Having visited the lakes at too early a season for obtaining nests and eggs of the mountain, or land-birds, I was able to observe very little as to their manner of constructing their nests. On the Klamath river I saw immense numbers of long-winged swallows (the name unknown) building on the sides of rocky cliffs. Clark's crow and wild pigeons were also engaged in building nests; both of them are to be found in large numbers about that part of the country; also, plover, snipe, and divers, but no nests could be found; yet I am told they will build about the middle of June.

NOTES UPON SOME ANIMALS OBSERVED DURING THE EXCURSION.

MAMMALS.

Little prairie dog.—I saw this little animal for the first time about four miles south of Fort Tejon, California, on a small plain, and met with it again in Butte Creek valley, on the south side, where they were quite numerous. It has the same habits as the prairie dog found on the great western plains. The latter lives in separate holes; the former associate together, several living in the same hole. When approached, they give a signal, which can hardly be called a bark, but a single "zeek;" this signal is repeated by all, and then a general scampering for holes takes place; arriving at which, and out of harm's way, they sit on their haunches, like a squirrel, and look about to reassure themselves of their perfect safety, all the time continuing their "zeck." Should they suspect danger, they [illegible] quickly into their holes, and there remain for about ten minutes, until all is [illegible], when, cautiously venturing out, they resume their gambols.

Moles.—Of this family I had two kinds, and, in fact, the finest I ever saw; [illegible] on Bogus creek, with glistening silver-gray fur, the other on Klamath [illegible] with black fur and velvet-like appearance. They were of the same size [illegible] common mole. Both of them I caught towards evening, when they [illegible] in the ground. These two moles and five prairie dogs I lost in [illegible] the Indians.

BIRDS.

Picus albolarvatus, (white-headed woodpecker.)—This bird is abundant in the high timber between Fort Crook, California, and Shasta Butte mountain. I saw it very seldom about inhabited places. It hunts, like all woodpeckers, in the bark of trees for insects, and generally in pairs, by commencing almost on the foot of the tree, moving in spiral circles up on the trunk, keeping from time to time a shrilling note to tell its companions its whereabouts. Its flight is an up-and-down movement, like that of most woodpeckers. I never saw it in pursuit of flies or insects in the air, like so many of its tribe.

Picus harrisii, (Harris's woodpecker.)—This species, in its habits, is closely allied to the first mentioned, except in not avoiding inhabited places, as I have often seen it in search of food on the pillar of my porch and adjoining fence. It will frequently be seen moving downwards on a tree, keeping the body in an upright position and moving sideways and downwards—the head always up, never down, in these movements. Its notes are similar to, but sharper than, those of *Picus albolarvatus*, which it also resembles in flight. It seldom gives a note while engaged in search after insects, but generally, when leaving a tree, it repeats it until it has alighted on another place. It inhabits this country during the whole year, and cares very little about bad weather. I have seen it in rainy and snowy days, and late in the evening, busily engaged in the destruction of worms and insects.

Collyrio borealis, (great shrike.)—This bird is generally to be seen on the borders of timber, about brush, where it will always select the highest dry branch. Very few are to be seen about Fort Crook, California, but in Shasta valley, about the cedar brush, near Sheep Rock, I found it quite abundant, and pugnacious as usual—at war with the larger birds to keep them away from its haunts, and the smaller ones to kill them for food. Birds and grasshoppers are its prey; of both killing more than it can consume, and fastening the "surplus" to a thorn or brush, there leaving it to decay. Judging from the number of crickets I saw transfixed to thorns, the sanguinary habits of this bird should be commended rather than condemned, as they prove useful agents in clearing the country of one of its greatest plagues.

Certhia Mexicana, (Mexican creeper.)—This bird is very abundant in spring and fall about Fort Crook. I found it almost everywhere, and cannot say whether it prefers the vicinity of pine or of oak trees. Busy in its solitary movements, it would be seldom observed but for the frequent utterance of its lonely three notes. the first sharper than the two following. Its color corresponds very much with that of the bark of the trees. I always saw this bird moving upwards, never head downwards, and going in spiral circles or straight lines up a tree; instead of a kind of jumping, like woodpeckers, their movement on a tree is a kind of running. The approach of a man will not drive them off. I have often attempted to catch them with my hand. If accidentally seen by them, or if a shot be fired close by, it will often stop on the same spot for some time without giving a sign of life; at other times it will turn on the trunk of a tree on the same side, close to the intruder; but once started, its flight is quick. It is generally found in company with the titmouse and nuthatches.

Sitta aculeata, (western nuthatch.)—This bird inhabits this part of country during the whole year, but is not so common or abundant as might be expected. I often travelled in the forest for whole days without observing one. They generally move in pairs, and will be noticed from a long distance by their singular "quak, quak." It is amusing to see this little bird, so full of life and activity, moving upwards or downwards or sideways, on top or on the lower side of a limb; in fact, taking every imaginable position to secure ants and insects in or upon the scaly bark of trees.

Sitta canadensis, (red-bellied nuthatch.)—This bird resembles in its habits the *Sitta aculeata*, always moving up or down in spiral circles on the trunk of a tree, upon or on the lower side of a limb, in search of ants, insects, &c., frequently repeating its note "quank," a good deal sharper than that of *Sitta aculeata*, and keeping mostly in the dark, distant forest. I often saw it on the point of a dead tree flapping the wings by turning in every direction and making a whistling noise. I found this bird more cunning than the preceding species; their quick movements and great care to avoid the hunter's sight makes it often troublesome to shoot one. It inhabits this part of country during the whole year.

Sitta pygmæa, (the pygmy nuthatch.)—(Nos. 5, 22, 33, 34, 102, 199, 200, of my collection forwarded.) This bird is very abundant in this section of country, and in color and habits almost like *Sitta canadensis*. When I first observed it, I supposed it to be a young bird of the species just mentioned, but by closer observation I soon found it to be distinct. More than one pair of *Sitta canadensis* are seldom found together, whereas this species travels in flocks numbering sometimes twenty and more. accompanied by titmice, *Certhia mexicana*, *Sitta canadensis*, and *Sitta aculeata*, and instead of the note "quank quank," it has a chattering whistle. It usually searches for its food only on branches and limbs; *seldom* will it be seen moving on the trunk of a tree. The pine nuts are very closely searched for their seeds; when found, it alights on a limb, where, holding it with one foot, it hammers with the bill until it has broken it in such parts as to enable it to eat its seed. If it should happen to one to drop such a seed, two or three will be seen diving after and catching it before it can reach the ground; another place will be found, and the hammering commences afresh. The scene presented by observing a party of these little birds all in a bustle and activity, engaged in breaking pine nuts, and to hear their chattering and hammering, is very interesting, and reminds one of an immense machine shop, where all the mechanics are busily engaged at the various divisions of their craft.

This bird is not so much afraid of the approach of a man as *Sitta Canadensis*, and I have seen it almost every day on the trees about the post; but at breeding season—June, July—it removes to the thick forest.

Pipilio chlorura, (Blanding's finch.)—This bird generally inhabits mountains barren of trees, and only covered with chaparral, always keeping close to the sheltered thickets, where it searches among the dead leaves, &c., for its food. About Shasta butte, (northeast side,) they might be called abundant, but I never saw more than one pair together. Upon alighting on the top branches of a bush it utters a short, sweet, and lively song; if alarmed when so engaged, it dives into the brush and disappears from sight, and is then very difficult to shoot.

Junco oregonus, (Oregon snow-bird.)—I found this bird to be very abundant throughout the whole of California. On my route to Rhett lake, &c., I met with large flocks among the brush or underwood, searching on the ground among the dead leaves for their food, uttering a chirping noise. When alarmed they quickly take to the thickets or trees; but if no further danger is apprehended they come out again and continue their search. When undisturbed they are very familiar, sometimes coming almost close enough to the observer to be touched with the hand.

Xanthocephalus icterocephalus, (yellow-headed blackbird.)—This bird arrives early in spring in this part of the country, where it remains until the breeding season, in June; then it removes further north, where it selects the margins of the lakes or swampy valleys covered with tules, in which it builds its nest. This is placed about six inches above the water, and is formed like a basket, by twisting dry or swamp grass around several pieces of tule until it has formed a nest. It generally lays four eggs. While the female is setting, the male,

particularly in the morning and evening, does his best to please his mate with his song, which much resembles the creaking of a gate in want of oil. Should a hawk approach their breeding place, the alarm is given and hundreds will be seen fluttering about the intruder, less with intention to fight than to annoy him with their noise, which they continue until he has left.

Several nests sent on by me are of another species of blackbirds, which breed on the ground in little brushes or bunch-grass, but I am not able to give their names.

Picicorvus columbianus, (Clark's crow.)—This species I first observed about Pitt river, and on Stoneman's ridge, in very small numbers, but on the mountains between Klamath river and Butte Creek valley I met with them in abundance, flying from tree-top to tree-top, like a jay, emitting from time to time a note or cry resembling very much that of a crow, and hammering on the bark of trees and on pine nuts, upon which it very probably feeds. It is a very shy bird and difficult to secure. On opening some of them, towards the end of May, I found quite large eggs formed in the female; but I never saw a nest.

Cyanura stelleri, (Steller's jay.)—This jay, the most cunning of its tribe, I met with everywhere in the timber or brush along rivers or creeks. On the approach of any intruder it utters a vehement outcry, jumping from limb to limb, and from one tree to the next, until out of sight. In addition to this single note, this bird has the faculty of mimicking other species with great exactness. Jays, in general, are active, lively birds, but this species is superior to all others in California, feeding indifferently on the ground or on trees. During the summer season it keeps constantly in the forest, but heavy falls of snow compel it to seek food and shelter about the settlements, and then it is easily caught in traps. When caged it soon becomes tamed, and eats freely of meat in preference to a vegetable diet.

Gymnokitta cyanocephala, (Maximilian's jay.)—The first time I saw this bird was at Fort Tejon, California. They generally fly in flocks, keeping on or along the mountains, and, when so moving, their notes, which are almost pleasing, are frequently given forth so as to guide the stragglers; but when in search of food they are perfectly quiet, and the start for a new flight is announced by commencing their notes again. They generally fly very high, and feed mostly on cedar-berries, but also search on the ground for food. It is difficult to approach and shoot them wherever they are, as they are always on the lookout; in fact, all that I killed were obtained by secreting myself under a tree, &c., on a place where I had previously seen some pass, those in the rear always following in the same direction, guided by the notes of the party in advance. One day I observed a flock drinking. The whole of them first alighted on a tree near the creek, and finding all quiet, about four of them took to the ground and satisfied their thirst. When these were done they returned to the tree; then all the others followed their example. By this arrangement some were on guard all the time. No sound or note was uttered until all had done, then the flight and noise recommenced. In the winter of 1858 I caught several of them; in fact two came of their own accord into my room and made themselves quite at home, which astonished me very much, because I had found this bird to be very wild. The intense cold, however, and the covering up of their food by the snow, compelled them to take refuge about our quarters until the snow had gone from most of the country.

Perisoreus canadensis, (Canada jay.)—This bird I first saw in 1859, in small numbers, when on an Indian scout, about Lossen's butte, hovering noiselessly about the road, catching flies by darting from one tree or bush to another. Their flight is light and easy. About Shasta butte (north and east side) I found them in large numbers, up to twenty together, noiselessly and busily engaged, and searching for insects on the ground. Here I shot and preserved

several, but could only save one specimen. They appear to prefer the snowy region, well suited to their long and downy feathers, and I never met any elsewhere. Some of the jay tribe known to me I saw engaged in their habit of catching insects in the air. They differ from any others of their tribe that I ever met with.

No. 308 *of my collection*, (not identified.)—The first time I observed this bird was at the station-house on McCloud's river, where it was in large numbers. At first sight I thought they were cross-bills, owing to their manner and whistling note, but by closer observation found the differences in size and color. The whistling noise of so large a party can be heard a considerable distance off. Their flight is swift, and when flying their wings make a surprising noise. I sat under a willow bush in Butte valley engaged in cleaning eggs, when I suddenly heard a noise in the air as if several hawks were diving at their prey, and before I had a chance of leaving my place to see where the noise came from, all the willows were covered with these birds, whistling and jumping in a very lively and active manner, and feeding on the willow buds. I had previously obtained two specimens only of this species, (shot at the station-house on McCloud's river,) and here having so fine a chance for more, killed seven specimens at one shot. All the others, with their usual noise, took flight to the mountains. On opening the females I found quite large eggs already formed.

Numenius longirostris, (long-billed or brown curlew.)—This bird is seen at Fort Crook during the spring, but only while migrating north; but in Butte valley, particularly the western portion of it, which is swampy, they appeared to be numerous, eggs and nests being abundant. The nests are built of dry grass, placed in a hollow previously made in the dry ground. The nests usually contain four eggs, nicely arranged, with the pointed end towards the centre. This bird makes a singular noise, something between the cooing of a dove and the whistle of a quail. When driven from the nest by the hunting of the dog it displays great sagacity by leading the dog a circuitous path from the nest for some distance and then suddenly flying off. At first it hops or flutters along like a young bird, and just when the dog is about to pounce upon it, off it flies, uttering its note, cooi, cooi, sounding like a contraction of go away.

Fulica americana, (mud-hen, or coot.)—This bird, common throughout this State, is particulary so in Butte lake, where there is no difficulty in procuring immense quantities of eggs and nests. I have myself seen a canoe-load of eggs collected in less than an hour's time. The nest of the bird is built on the water, and, as its construction is somewhat novel, I will endeavor to describe it at length. The tules bordering the waters are bent down, one towards another, so as to form a base or foundation; then other tules are interwoven, adding strength to the structure, and the whole is finished off by an interlacing of the same material. The whole, when finished, has the appearance of an inverted cone. The nests generally contain from nine to eleven eggs, and present the appearance of a floating basket.

Ducks.—I never paid much attention to the ducks, owing to the vast number found over the whole country. During my trip to the lakes many opportunities presented themselves for becoming acquainted with their manners, habits of breeding, &c., but I was prevented from availing myself of them on account of the Indian difficulty spoken of in my letter. The most favorable season for collecting eggs and nests is the end of May and first of June. Ducks generally select high grass, or little brush, on the skirt of water, lakes, rivers, &c. The nest will be easily found after detecting the trail leading from the water. The duck never alights near the nest, but at some distance from it, in order to avoid notice; after alighting in the water it swims to a spot near the nest, and then, walking through the grass always from the same point, makes the trail

spoken of. Several eggs and nests have already been forwarded, but I am unable to class them, as the bird was not seen on the nest.*

Humming-birds.—This family is well represented about Yreka and vicinity. I found several nests, in all of which the eggs contained embryos. The nests I had were of three kinds: those found on willow bushes *(Atthis anna)* were lined with spider webs, those on oak trees *(Callothorax calliope)* with small scales of bark, and some found about Yreka on small bushes were coated on the outside with a combination of small leaves and scales of bark, bound together with spider webs. Of birds I only obtained two. The head and neck of one were of a very brilliant fire-color, *(Atthis anna,)* the other resembling the *Callothorax calliope*, but with a longer bill; this latter one was abundant about the willow brush at Butte Creek valley. Both nests and birds I lost in the difficulty with the Indians.

INSECTS.

Grasshoppers.—These insects were first seen by me in Pitt River valley during the summer of 1859, and in such number as actually to cover the ground. They were confined to the west side of the river, which divides the valley, none being seen on the opposite side save a few, which, having fallen into the stream, were carried over. So numerous were they that vegetation was entirely destroyed throughout the valley on the west side of Pitt and Fall rivers.

The eggs are laid in July and August. The insect deposits the eggs by making perpendicular and oblique holes in the ground, to the depth of an inch, by means of its tail, which is shaped like a bayonet, and is hollow. The eggs are passed from the ovary into this tail, and are dropped one by one into the holes. The localities selected for making these nests, or holes, is generally on ground slightly undulating, the nature of the soil making little difference; the nests are generally made on the south side of the hillock or knoll. Some six eggs are laid in each nest. The eggs resemble, in form, pieces of vermicelli of one-fourth of an inch in length, and are of a milky-white color. After making some three or four nests, or holes, in the manner described, the insect dies, seldom living more than twelve hours after the last deposit. It appears to have no particular food, but feeds upon all kinds of vegetation; it will eat the dead and crippled of its own kind, but I never observed them destroy one another. When caught in the hand they bite, and emit a very disagreeable, dirty, dark-green fluid from the mouth. It is remarkably tenacious of life; I cut the head off one, and could distinctly see signs of animation some twelve hours after. There is but little difference in size between the sexes. Immediately before uniting sexually, the insect without the tail (which I presume to be the male) utters a shrill, whistling sound, as if to call his mate. The sexual act lasts about one or two minutes, and the peculiarity I observed at this union was, that the one which I presume to be the female was over the male, instead of the reverse. After the act a small bag—evidently the ovary—is attached to the body of the female close to the tail; this is extracted from the other without the tail; after a while the bag disappears. The time which elapses between the sexual act and the deposit of the eggs I am unable to state, but after the eggs are deposited it is my belief that they remain until the following spring before being hatched. Their migratory flight extends but a few miles. When moving, they start in the morning, and, from their precision of movement, they appear like a vast army on parade. The course once marked out, they never deviate from it on account of any obstacle, but move straight forward over houses and all else. Clear and warm days are chosen for travel, but the too scorching rays of the sun will cause them to seek shelter, as will also inclement weather; the cover usually selected is bunch-grass or low brushwood, &c.

* Those identified belong to the red-breasted teal, *(Querquedula cyanoptera.)*—S. F. B.

In changing the skin the insect fastens itself on a tree or other object, head downward, and after much exertion crawls out, head foremost, leaving, to all appearance, its very counterpart behind; they now present themselves in a suit of light green, in exchange for the dark brown. The only effectual remedy against the ravage of this insect is the "cricket-fence," and this structure, simple as it is, has proved the only barrier.

The crickets were seen in numbers in the vicinity of Yreka, and as far on my route as Bogus mountain. Thus it will appear that they are not a universal plague in this section of country, but visit certain localities only. From what I could learn from the Indians, the cricket makes its appearance every three years, and I was also informed by the Indians that Shasta valley is known as "Cricket valley;" hence it would seem that Shasta valley is their range.

[The intrepid explorer whose notes we have here published, and to whom the Smithsonian Institution is indebted for a large number of valuable specimens of natural history, fell a victim to the treachery of the Indians. The details of his fate are given in the following report of General Sully, on whose staff he served as engineer:

"On the 28th June, 1864, we reached the Little Shyenne. Captain Feilner, 1st United States cavalry, with two detailed men, proceeded ahead of the column to the creek of the Little Shyenne, and reached it at the same time a party of my friendly Indians and half-breeds did, but about half a mile below where they were, and hidden from their view. The column had not yet arrived, and as it had been raining hard the night before there was no dust visible. A small war party of Minnesota Sioux were camping in a thick cluster of brush and trees where the captain dismounted, and while he was in the act of getting some water, three Indians fired about six yards from him. Two of the shots took effect, and the captain died in great agony about two hours afterwards. I am thus deprived of my engineer officer, and the country of the services of one of its most valuable and efficient officers. He had for many years served in the 1st United States cavalry—formerly 1st dragoons—as a non-commissioned officer, had been in a great many battles, and it is sad that he should lose his life in this way. It was all owing to his enthusiastic desire to collect as many specimens as possible for the Smithsonian Institution. I had cautioned him several times about the risk he ran in going so far from the command, and on the night previous to the day of his death I sent for him to my tent to talk to him on the subject, and I offered him a party of my scouts to protect him, as I was desirous that he should do all he could to forward the scientific researches that he was sent here to attend to; he promised me to accept them, but did not do so."

J. H.]

JOURNAL

OF AN

EXPLORATION OF WESTERN MISSOURI IN 1854,

UNDER

THE AUSPICES OF THE SMITHSONIAN INSTITUTION.

BY P. R. HOY, M. D.

April 4, 1854.—Left Racine, Wisconsin, on the steamer Traveller. The day was fine, with a gentle breeze from the north. As the boat glided on, large numbers of herring gulls, *Larus argentatus*, followed in her wake, picking up, with clamorous cries, whatever would serve their gluttonous appetites. I noticed one ring-billed gull, *Larus zonorhynchus*, a lesser, more active, and graceful species. Saw a scoter duck, *Fuligula americana;* this marine species is occasionally met on Lake Michigan during winter and early spring.

5th.—Stayed in Chicago last night. In the forenoon we had a pleasant ride on the Galena railroad to Rockford, on Rock river. The train passed close by a flock of wild geese, which appeared more surprised than frightened, arranged soldier-like in a long line. They silently gazed with upstretched necks, looking as if they would demand "What next?" From Rockford we went, in the afternoon, by private conveyance, to the residence of Hon. S. Ruggles, near Grand de Tour.

7th.—Rigged out our "traps" and went on a fishing excursion to Pine creek, a small rocky mill-stream that empties its waters into Rock river. Caught a number of fish, some of which were new to me; captured a garter snake, *Eutænia sirtalis.* My son met with a hog-nose snake, *Heterodon platyrhinos*, but did not succeed in capturing it; the harmless serpent succeeded, by hisses and threatening attitudes, in intimidating the boy.

8th.—Went on Rock river to-day; caught several species of fish and a number of batrachians; nothing, however, new. The *Hylodes gryllus* is an abundant species here, keeping up almost a continual clicking rattle, which may be readily imitated by striking together two small pebbles, commencing slowly and increasing to a rapid chatter. I amused myself by "starting the tune," in which these diminutive castanet performers did not fail to join right merrily. Shot a coot, *(Fulica,)* a marbled godwit, *Limosa fedoa*, and several field sparrows, *Spizella pusilla.* Saw large numbers of geese and ducks. My son collected a fine lot of shells; some of the unios were most beautiful and perfect.

9th.—Shot many ducks, a swamp sparrow, and a grass finch. I saw a large number—I should judge not less than a hundred—painted tortoises, *Emys picta*, dead on the margin of a mill-pond, where they had been left by the melting of ice and frozen mud which had been dislodged and driven on shore by the spring freshet. A part of the pond was shallow and had frozen during the past unusually severe winter to the bottom, including a considerable stratum of mud in which the tortoises had crept to hibernate. Can any reptile be revivified after being thoroughly frozen? It is well known that fish will survive freezing, but

judging from several experiments made on serpents, I am led to consider reptiles too highly organized to recover after being *completely congealed.* In no single instance have I succeeded in restoring snakes to life that had been perfectly frozen. Under these putrefying tortoises I found numbers of beetles, *Oiccoptoma marginata.* Saw, to day, the first *Sylvicola coronata.*

11th.—Had a pleasant drive of thirty miles over a most beautiful prairie country to the "Junction," in La Salle county. Saw geese, ducks, sand-hill cranes, and golden plover in abundance.

12th.—Left the Junction at half past three in the morning, and in fourteen hours we were in St. Louis, distance 217 miles. At the Junction there was but little evidence of spring, vegetation having scarcely started, but, as we approached Springfield, the influence of a more southern climate was strikingly apparent, for the red-bud—*Cercis canadensis*—and peach tree were in blossom; and at St. Louis the pears, cherries, and plums are in full bloom. Such a change on the now smiling face of nature, since morning, is more like magic than reality. I noticed in a barber-shop window a *black-bellied fox squirrel,* just such as we find in the vicinity of Racine; on inquiring I learned that it had been obtained up the Wisconsin river.

13th.—Left St. Louis in the evening on the packet Honduras.

14th.—Making four miles an hour, including detentions on sand-bars. I saw this morning a number of cormorants; they must seldom be met any great distance from the mouth of the Missouri river, for we saw none above this. Saw a duck hawk fly to her eyrie in the face of an inaccessible cliff with a duck in her claws on which to feast her young.

15th.—We find ourselves, this morning, only ninety miles from St. Louis. Turkey vultures are nesting in the cliffs all along the river. The crows follow the steamboats for the purpose of picking up whatever is acceptable to their omnivorous craws, just like the gulls on the lakes. No gulls on the Missouri river.

16th.—Arrived at the residence of E. Elliott, esq., on the river, ten miles below Booneville, in Cooper county, where we propose spending a week.

17th.—Made a preliminary excursion to-day. The surface of the country here is much broken; soil on the hills good for wheat and most other small grain; corn and hemp raised principally on the river bottom. The timber growing on the hills is composed of various species of oak and hickory interspersed with sassafras of an unusually large growth; on the *bottom lands,* of cottonwood, sycamore, maple, elm, hackberry, (*Celtis,*) honey-locust, (*Gleditschia,*) coffee-bean, (*Gymnocladus;*) on the *hill-sides,* of mulberry and redbud matted together and overrun with grapevines. I was much surprised to find such fine old fruit orchards as we saw here. The trees are remarkably vigorous and healthy, free from the attacks of insects. One pear tree, 35 years old, on the "Elliot farm," produced the last season 45 bushels of excellent fruit. Peaches seldom or never fail to yield an abundant crop. Apples are now plenty at 25 cents per bushel, delivered; and better flavored, fairer, and more perfect fruit I never saw at this season of the year. Grapes do well with but little cultivation. All things considered, the hilly country bordering on the Missouri is one of the best *fruit regions* to be found anywhere. But few migratory birds have yet arrived, while vegetation is as forward as it is in Wisconsin on the 15th of May, but the crested tits and cardinal birds by their merry whistle do what they can to compensate for the deficiency of singing birds. Shot several squirrels; the *gray squirrel* here is no doubt distinct from the Wisconsin species; the *fox squirrel* found here appears different likewise. The want of unvarying characteristics by which closely allied species of *Sciuridæ* may be distinguished is to be lamented; for after all that has been done by Bachman and others in this department, it must be admitted that there is yet much confusion and uncertainty; we look with hope to the extensive collections being made by

the Smithsonian Institution for materials that will settle satisfactorily this perplexing matter. My son captured a number of insects, among which were several *Papilio ajax;* this species is numerous here, the papaw on which the larvæ feed being abundant.

18th.—Rigged out a team and went on a small branch of the Petite Saline to fish. After following the stream three or four miles and wading a good deal in the disagreeable muddy water, we had to return without even seeing a single fish; a sorry commencement in Missouri. We saw an abundance of the following species of frogs: *Rana pipens, R. fontinalis, R. palustris, R. halecina, R. sylvatica,* and *Hylodes gryllus.* Shot a Canada goose and a number of small birds. White-fronted and Canada geese are here remarkably abundant; they remain all winter and do much damage to the fall grain; one field that we saw, containing twenty acres of wheat, was quite ruined.

19th.—Went to Booneville; on the way we fished in a small brook and caught several interesting little fish—among them one beautiful species of *Etheostoma,* marked with bright blue and red bands.

21st.—Went on the river bottom; got one *parrakeet* and a fine little sturgeon; caught a garter snake, which had a row of red spots on the side of the neck, between the scales, such as I never saw before. In the afternoon we visited "*Sallie's Branch,*" a small tributary of the Little Saline; water muddy and brackish; caught but few fish; among them, however, were two species of *Pomotis* that probably will prove new. Wild turkeys plenty; came near getting a shot at a fine old gobbler. I obtained to-day a *Scotophis alleghaniensis,* 11 inches in length, the largest I ever saw; it was shot while basking in the top of a large oak, some 50 feet from the ground; there were two in company, apparently of equal size; the other escaped by crawling in a hole that was in the tree.

22d.—A black boy brought me a living Alleghany black snake, which we released near the root of a tree. The serpent, seeing that it was surrounded, commenced ascending the trunk, but before it got out of reach I pulled it down and removed it some 15 feet, when I again let it go; the snake finding itself at liberty, elevated its head and took a survey of the enemy's position; retreat being cut off by the crowd of spectators, it started at full speed for the tree, ascended to the first horizontal branch, on which it ran out to the extremity, then elevating its body succeeded in reaching the next branch above, and so on until it had ascended over the small outer branches to the top. The ease and rapidity with which this snake climbed the tree proved that such a feat was nothing unusual. In this instance the snake evidently took to the tree for the purpose of escape. This species *(Scotophis alleghaniensis)* is common among the bluffs along the Missouri river, *in the timber only,* while the blue racer *(Bascanion constrictor)* was repeatedly found on the prairies a considerable distance from any timber; hence we infer that the latter is less strictly arboreal in its habits than the *Scotophis alleghaniensis.* I obtained here a most beautiful and unusually colored specimen of *Celuta amœna (?);*—also specimens of *Leptophis æstivus.* Mr. Judson caught for me, a few days after we left here, an *Elaps fulvus,* (perhaps *E. tenerus.*) Is not this the most northern locality in which this beautiful southern serpent has been discovered? Shot a *Vermivora pennsylvanica* and *V. solitaria.* A few of the advance parties of *Sylvicolinæ* begin to make their appearance.

25th.—Early this morning we went on board the Michigan, a splendid large "lower river boat," bound for Lexington, distance 125 miles, where we expect to arrive to-morrow noon.

27th.—We have been three days on the boat and only 75 miles on our way; river low; steamer too large; said to be the largest that ever ascended the Missouri; and, judging from the vexatious detentions on sand-bars, is likely to remain the largest.

28th.—Made eighteen miles to-day. Great numbers of geese on the islands,

mostly the white-fronted species, here called "blants." Ducks not numerous; those we saw were mostly wood ducks.

29th.—At Hill's Landing this morning; took a stroll through the woods while the boat was being "lightered" over the bar; found a dead snake, about four feet in length, such as I never saw before; color above, greenish olive; beneath, reddish yellow; scales on the sides smooth; those on the back slightly carinated. I regret that the head was so bruised and lacerated as to preclude an examination.

30th.—Reached Lexington in the afternoon, glad enough; drove directly to the residence of my brother, J. D. Hoy, esq., who has been for many years a resident of Missouri. He is to accompany us in all our excursions in Missouri and Kansas.

May 1st.—Rainy day; occupied in looking over the specimens from Cooper county.

3d.—Fished to-day in a small "branch" near the town; caught only a few fish. Warblers are not yet abundant; mostly males; the females have not yet come.

5th.—Drove 12 miles to the Tabo, a miserable, muddy, sluggish, fever and ague stream. Suckers, catfish, shovel fish, and gars are about the only species that can live in such villanous water. My son shot a fine male *Vermivora prothonotaria*.

7th.—Shot a mourning finch, *Zonotrichia querula*. Lincoln's finch is a common species here.

9th.—Started for Utica; had to return, as the wind was too high for the ferry-boat to venture.

10th.—This morning we were enabled to cross; had a charming drive of 40 miles over one of the most beautiful countries I ever saw, mostly high rolling prairie in a state of nature; much of this rich land still remains subject to entry at government price. The black-throated bunting is almost the only bird that inhabits these prairies. Had a spirited chase after a blue racer, which finally made good its escape by crawling into a "gopher mound." Night overtook us before reaching Utica. We accepted an invitation to remain until morning with Colonel Gregory, a gentleman of wealth, formerly from North Carolina.

11th.—At an early hour we arrived at Utica, a small thriving village pleasantly situated near the forks of North Grand river, in Livingston county, directly on the line of Hannibal and St. Joseph railroad, now in course of construction.

12th.—Fished with the minnow net in Grand river below the mill; caught great numbers of interesting specimens. Shot a *Nerodia transversa*, a rare species of water-snake, heretofore only found in Arkansas; it was basking on some drift-wood when it was discovered by one of the ladies of our party, who was wonderfully frightened, while I was greatly delighted. Such is education that an object of fear, and even disgust, may be converted into a source of delight, capable of exciting pleasurable and profitable reflections, thereby opening *new sources of rational enjoyment*, one of the highest aims of education.

13th.—Crossed the river and drove to Chillicothe. Shot many birds, among them a mourning finch; there were 15 or 20 in company, but it being near night I only obtained a single specimen. The warblers are now here in their greatest numbers; it is remarkable that there is so little difference in the first appearance of migratory birds here and at Racine, Wisconsin—certainly not more than three days at most, while vegetation here is not less than three weeks in advance of Racine. Made arrangements to have a large seine drawn in the river to-morrow.

14th.—Rained all last night, which puts an end to our fishing here; this I greatly regret, for there are several species of fish I am exceedingly anxious to obtain, especially one species of *salmo* (?) called here *salmon trout*, a short-billed gar, and the padde fish, which are represented as being abundant. Grand

river is the first stream we have seen in Missouri that is tolerably well supplied with fish. A lad caught on a hook to-day a catfish weighing 136 pounds. Skinned a fine old gobbler shot by a friend; wild turkeys are plenty in the vicinity.

16th.—Went on the extensive "bottoms" of Grand river, so celebrated for rich land and heavy timber; we found the principal forest trees to be black walnut, burr oak, cottonwood, sycamore, hackberry, shagbark hickory, pecan, coffee bean, honey locust, and black birch, all of which grow to an unusually large size. We measured the trunk of a pecan, *Carya olivaformis*, that was 13 feet in circumference, and held this size for at least 50 feet. Parrakeets are abundant about the large sycamores, *Platanus occidentalis*, in the hollows of which they roost and nest. Here we found the *home* of the *Trichas philadelphia*, a locality where this bird is common; they frequent localities covered with dense underbrush overrun with climbing roses and honeysuckles. Here, too, in the same localities, is found the *Kentucky warbler* in great abundance; I spent considerable time watching these active, restless little songsters, and I must confess my surprise that any naturalist who had an opportunity of observing the *behavior* of these birds, especially during their nuptial season, should hesitate a moment in placing them among the *ground warblers*. They live and nest in the underbrush, the male occasionally hopping upon a low branch of a tree to pour forth his *whittesheé, whitteshee´*, repeated two or three times, then again disappearing in the tangled brush. This song is so precisely like that of the yellow-throat that it requires a practiced ear to distinguish the one from the other. I listened to the song of the *Trichas philadelphia*, *T. marylandica*, and *T. formosa*, at the same time, and found them wonderfully alike; the song of the *T. philadelphia* differing more than the Kentucky warbler's from that of the Maryland yellow-throat. The *color*, the rounded tail, destitute of the white marking constantly found on the lateral feathers of the true sylvicolas, the flesh-colored feet and legs, all combine to establish the *Kentucky warbler* in the genus *Trichas*. Specimens of *Vermivora solitaria* are common here; they keep much on the tops of trees; their song, *zeé-z-z-zee*, resembles closely that of the *Vermivora chrysoptera*.

17th.—Went hunting *salamanders;* turned over a "world" of old logs, but found none. Is it not strange that we did not find a single specimen of this animal in all our travels, notwithstanding we hunted diligently in every favorable locality? Shot two woodchucks, *Arctomys monax*, as they were running full chase through the woods. There is here a "gray prairie squirrel," *Spermophilus;* we did not obtain a specimen.

18th.—Started on our return to Lexington; drove 29 miles, and put up with the Rev. Mr. Grover, an intelligent, energetic "planter." He gave me much information respecting the habits of the "gopher," *Geomys*, which are greatly abundant here. On the prairie we passed at no great distance from a pair of cranes, *Grus canadensis*. My brother waved his hat and shouted to them two or three times, when the male bird commenced, by bowing and hopping in a ludicrous manner,—a series of amusing antics, interluded with brief samples of vocal *powers* that made ample compensation in *strength* for any lack of melody.

19th.—Early this morning, in company with the Rev. Mr. Grover, went a short distance on the prairies to kill "gopher," but were disappointed in not getting a shot. Arrived at Lexington in the afternoon; we halted on the road to pick ripe wild strawberries.

24th.—Went six miles above Lexington to fish in the Sny; caught but little. A person accustomed to the beautiful clear streams of Wisconsin, literally filled with fish, experiences great disappointment in visiting the muddy saline streams of Missouri. We got a fine old "timber rattlesnake," *Crotalus durissus*. If I mistake not, it is generally believed this snake will not take food while in captivity. Mr. S. Sercomb, at Madison, Wisconsin, has succeeded in feeding his "pet rat-

tlesnakes" with *living birds*, which they kill and swallow readily; they will not touch frogs, or even dead birds.

25th.—This afternoon J. D. Hoy, my son, and I started in a two-horse carriage to visit the head-waters of the Osage river. Drove twelve miles through a well cultivated rich hemp-growing section, and put up with William Hooks, a whole-souled Missourian, *house* and *heart* open. Caught two rare little frogs.

26th.—Travelled to-day through the rain; luckily it cleared away just long enough for us to get a good view of the great eclipse. Near Chapel Hill, in a dense thicket of thorns, I first heard that charming little songster *Vireo bellii*. I had much difficulty in procuring specimens, for the restless little birds kept flitting from one point to another continually, all the while warbling forth their agreeable song. From this point south we heard this bird repeatedly. Put up during the night at Pleasant Hill, Jackson county.

27th.—This morning we went four miles to witness the drawing of a seine in the north fork of Grand river. Obtained several interesting fish, and a fine lot of terrapins, embracing six species. Here we saw the rose bug, *Melolontha subspinosa*, in immense numbers, the willows being literally stripped of everything green by these pests of the agriculturist. In the afternoon we drove directly south over the prairie; stopped on Elm creek to fish. While engaged in the stream a number of our terrapins took to their legs and made good their escape; to our sorrow three of the species left no representatives to be forwarded to the Smithsonian Institution. Here we caught a species of *Astacus* different from any I ever before saw. Put up at Dr. Maxwell's, three miles north of Harrisonville, Cass county, (formerly Van Buren county;) in the evening I shot a *Vespertilio noveboracensis*.

28th.—Remained with the doctor. I caught a straw-colored snake unknown to me. We learned here that there are two species of "*prairie squirrels*" inhabiting the prairies in the vicinity—one *gray*, the other red, called "prairie fox squirrel;" we did not get specimens.

29th.—Rain, rain—continual rain! We held a council of war, and decided that the water in the streams was so high that it would be impossible for us to proceed on our contemplated route to the Little Osage, then down, *via* Osceola and Warsaw, to Versailles. Being thus headed off, we concluded to strike southwest into Kansas. We crossed the main Grand river at a mill, where I was told bats inhabited the old frame. After a deal of punching about the angles of the braces, I dislodged an old acquaintance—*Vespertilio subulatus*. There is a larger species inhabiting the woods here, with "prodigiously long ears," called *mule bat*. This is the most northern point I heard of them. They are abundant about Osceola, St. Clair county. From this we drove for twenty-five miles over a delightful prairie, covered with beautiful wild flowers, among which the gay-colored *Phlox glaberimum* was particularly conspicuous. We drove by *direction*, regardless of roads. I was many times apprehensive we should get into trouble; but my brother's practiced eye never failed to *pilot* us to the desired point. We reached the residence of Mr. Clymer, near the State line, late in the afternoon.

30th.—Drove directly west six miles into Kansas, until we came to Sugar river, a branch of the Marie des Cygne. The land on this beautiful stream is of a superior quality, and the fine sugar-maple groves gave a homelike appearance to the landscape. We caught a number of fish, notwithstanding the stream was rather large and flushed by the recent rains for our small nets to do good service. We were told this stream was celebrated for the large number of fine fish its waters afford. Turkeys and deer were abundant. We ate our lunch under the ample shade of a sugar maple, cheered by the song of a mocking bird and a *Vireo bellii*. From Sugar river we drove directly north, on the old military trail, which leads over a well-watered, rich section of country. On the way I saw the only prairie reed-bird (*Dolichonyx bicolor*) I ever met. I followed it, in full chase, under a hot sun, at least two miles before I shot it. Although greatly

fatigued, I was well satisfied at my final success in obtaining the much-coveted bird. Arrived in the evening at Poge's, on the line, thirty miles from Clymer's—the first house we have seen since morning. We saw in several places where the prairie wolves had recently been digging burrows in the gopher mounds. At the time, I supposed they were after the gopher, but was informed by our host that they were digging these burrows for the purpose of concealing their whelps. A few days since there were two badgers caught in this neighborhood.

31st.—Heavy rain all night, accompanied with much lightning and thunder. The storm made our otherwise uncomfortable quarters still more unpleasant. Cranes are only found here during fall and early spring, when vast flocks frequently visit this locality. Poge told us he shot two that differed much from the common species, the head being covered entirely with feathers, instead of the bare red skin always found on the common species. One of these was so slightly wounded that they kept it alive for some time. When angry it would erect the feathers on the crown. These were in all probability the *Grus hoyanus*, a new species recently found in Wisconsin. From Poge's we struck a bee-line for Independence. The heavy rains have swelled every little rivulet into almost an impassable stream.

June 1st.—Put up last night at Independence. Started at an early hour for Lexington. I succeeded to-day in shooting a *Geomys bursarius* while he was unloading his sacks at the mouth of his hole—the first I have been able to obtain, notwithstanding we have been continually where their works were abundant. As a natural history item, I record the measurement we took to-day of the "Missouri giant ox," raised by H. S. Bellos, of Rochester, Andrews county, Missouri: Length from elbow to top of withers, 3½ feet; from knee to point of shoulder, 3 feet; heel to point of hip, 4½ feet; from brisket to top of shoulder, 5 feet; around the knee, 22 inches; height, 19 hands, (6 feet 4 inches;) length from point of nose to insertion of tail, 12 feet; age, 6 years; color, red; breed, native. We saw the skins of two beavers that had been recently caught on an island in the Missouri river, ten miles above Lexington. We had intended to visit the spot to view their recent works, which were represented as being a great curiosity. Arrived at Lexington in the evening. This day is the first that it has failed to rain since we left. In our absence Mrs. Hoy caught a second *red-marked* garter-snake.

2d.—Occupied in packing and forwarding our collection to the Smithsonian Institution, our expedition being brought to a close.

4th.—Started home, where we arrived after a rapid and pleasant journey by way of Ohio.

The following list embraces all the species of birds observed by me in western Missouri:

A list of birds noticed in Western Missouri, above Boonville, between April 16 *and June* 15, 1854.

Cathartes aura.
Haliætus leucocephalus.
Pandion haliætus.
Falco anatum.
Tinnunculus sparverius.
Astur atricapillus.
Accipiter fuscus.
Accipiter cooperii.
Buteo borealis.
Buteo lineatus.
Archibuteo sancti johannis.
Nauclerus furcatus.
Circus hudsonius.
Bubo virginianus.
Syrnium nebulosum.

Antrostomus vociferus.
Chordeiles virginianus.
Progne purpurea.
Cotyle riparia.
Chaetura pelasgia.
Ceryle alcyon.
Lanius ludovicianus.
Tyrannus intrepidus.
Tyrannus crinitus.
Tyrannula virens.
Tyrannula fusca.
Tyrannula acadica.
Setophaga ruticilla.
Setophaga wilsonii.
Culicivora cærulea.

Vireo flavifrons.
Vireo solitarius.
Vireo gilvus.
Vireo bellii.
Vireo olivaceus.
Icteria viridis.
Mimus polyglottus.
Mimus rufus.
Mimus felivox.
Turdus migratorius.
Turdus mustelinus.
Turdus wilsonii.
Sciurus aurocapillus.
Sylvicola coronata.
Sylvicola petechia.

Sylvicola æstiva.
Sylvicola maculosa.
Sylvicola virens.
Sylvicola blackburniæ.
Sylvicola icterocephala.
Sylvicola castanea.
Sylvicola striata.
Sylvicola americana.
Sylvicola canadensis.
Sylvicola cærulea.
Trichas (Sylvicola) formosa.
Trichas marylandica.
Trichas philadelphia.
Vermivora pennsylvanica.
Vermivora peregrina.
Vermivora solitaria.
Vermivora protonotarius.
Troglodytes ædon.
Troglodytes hyemalis.
Troglodytes ludovicianus.
Regulus calendula.
Regulus satrapa.
Sialia wilsonii.
Certhia americana.
Sitta carolinensis.
Parus atricapillus.
Parus septentrionalis.(?)
Lophophanes bicolor.
Bombycilla americana.
Zonotrichia iliaca.
Zonotrichia melodia.
Zonotrichia pennsylvanica.
Zonotrichia leucophrys.
Zonotrichia passerina.
Zonotrichia pusilla.
Zonotrichia socialis.
Zonotrichia savanna.
Zonotrichia lincolnii.
Chrysomitris tristis.
Chondestes grammacea.
Euspiza americana.
Spiza cyanea.
Pitylus cardinalis.
Cocoborus ludovicianus.
Pyranga rubra.
Pyranga æstiva.
Sturnella ludoviciana.
Yphantes baltimore.
Yphantes spurius.
Dolichonyx oryzivora.
Dolichonyx bicolor.
Molothrus pecoris.
Agelaius phœniceus.
Quiscalus versicolor.
Cyanocorax cristatus.
Corvus americanus.
Corvus corax.
Trochilus colubris.
Picus pileatus.
Dendrocopus villosus.
Dendrocopus pubescens.
Dendrocopus varius.
Melanerpes erythrocephalus.
Colaptes auratus.
Centurus carolinus.
Coccyzus americanus.
Coccyzus erythrophthalmus.
Conurus carolinensis.
Ectopistes carolinensis.
Meleagris gallopavo.
Ortyx virginana.
Bonasa umbellus.
Tetrao cupido.
Rallus elegans.
Rallus virginianus.
Grus canadensis.
Ardea herodias.
Botaurus lentiginosus.
Ardeola exilis.
Charadrius marmoratus.
Charadrius vociferus.
Tringa pictoralis.
Tringa maritima.
Tringa pusilla.
Totanus vociferus.
Totanus flavipes.
Totanus solitarius.
Totanus macularius.
Totanus bartramius.
Numenius longrirostris.
Limosa fedoa.
Anser canadensis.
Anser albifeous.
Anas clypeata.
Anas boschas.
Anas strepera.
Querquedula discors.
Querquedula carolinensis.
Dendroncssa sponsa.
Fuligula marila.
Fuligula rufitorques.
Clangula albeola.
Mergus merganser.
Mergus cuculatus.
Sterna nigra.
Colymbus glacialis.
Podiceps cornutus.
Podiceps cristatus.

TABLES OF WEIGHTS AND MEASURES.

ENGLISH WEIGHTS AND MEASURES.

AVOIRDUPOIS.

	Grains.	Drachms.	Ounces.	Lbs.	Qrs.	Cwt.	Tons.
Grain	1.						
Drachm	27.34	1.					
Ounce	437.5	16.	1.				
Pound	7000.	256.	16.	1.			
Quarter	196000.	7168.	448.	28.	1		
Cwt	784000.	28672.	1792.	112.	4	1	
Ton	15680000.	573440.	35840.	2240.	80	20	1

TROY WEIGHT.

	Grains.	Dwts.	Ounces.	Lb.
Grain	1			
Pennyweight	24	1		
Ounce	480	20	1	
Pound	5760	240	12	1

1 cubic inch of distilled water, in air, at 62° F .... =252.456 gr.
1 cubic inch of distilled water, *in vacuo,* at 62° F .... =252.722 gr.

		Cubic inches.
1 gallon	=	277.276.
1 pint	=	34.659.
1 fluid ounce	=	1.7329.
1 litre	=	61.024.
1 cubic centimetre	=	0.061024.
1 cubic inch	=	16.387 cubic centimetres.

1.00000 parts of gas at 32° F., 29.922 bar., (also at 32°,) become, at 60° F., bar. 30 inches, (also at 60°) =1.05720 parts.

FRANCE.

METRICAL SYSTEM NOW IN USE.

LENGTH.

	English value.
Millimetre, (1,000th of a metre)	0.03937 inch.
Centimetre, (100th of a metre)	0.39371 inch.
Decimetre, (10th of a metre)	3.93708 inches.
Metre,* (unit of length)	39.3708 inches, or 3.2809 feet.
Decametre, (10 metres)	32.809 feet, or 10.9363 yards.
Hectometre, (100 metres)	328.09 feet, or 109.3633 yards.
Kilometre, (1,000 metres)	1,093.63 yards, or 0.62138 mile.
Myriametre, (10,000 metres)	10,936.33 yards, or 6.21382 miles.

* The metre is a ten-millionth part of the quadrant of the meridian of the earth, or, in other words, the ten-millionth part of the distance from the equator to the pole.

SURFACE.

	English value.
Centiare, (100th of an are, or a square metre)	1.1960 square yard.
Are, (square decametre and unit of surface)	119.6033 square yards, or 0.0247 acre.
Decare, (10 ares)	1,196.033 square yards, or 0.2474 acre.
Hectare, (100 ares)	11,960.33 square yards, or 2.4736 acres.

CAPACITY.

Millilitre, (1,000th of a litre, or cubic centimetre)	0.06103 cubic inch.
Centilitre, (100th of a litre)	0.61027 cubic inch.
Decilitre, (10th of a litre)	6.10270 cubic inches.
Litre, (cubic decimetre and unit of capacity)	610.2705 cubic inches, or 2.2010 galls.
Decalitre, (10 litres)	61.02705 cubic inches, or 1.7608 pt.
Hectolitre, (100 litres)	3.53166 cubic feet, or 22.0097 galls.
Kilolitre, (1,000 litres, or cubic metre)	35.31658 cubic feet, or 220.0967 galls.
Myrialitre, (10,000 litres)	353.1658 cubic feet, or 2,200.9667 galls.

SOLID.

Decistere, (10th of a stere)	3.5317 cubic feet.
Stere, (cubic metre)	35.3166 cubic feet.
Decastere, (10 steres)	353.1658 cubic feet.

WEIGHT.

Milligramme, (1,000th of a gramme)	0.0154 grain.
Centigramme, (100th of a gramme)	0.1544 grain.
Décigramme, (10th of a gramme)	1.5440 grain.
Gramme, (unit of weight)	15.44 grains.
Decagramme, (10 grammes)	154.4 grains.
Hectogramme, (100 grammes)	1,544 grains, 3.2167 oz. troy, or 3.5291 oz. avoirdupois.
Kilogramme, (1,000 grammes)	$32\frac{1}{8}$ oz. troy, or 2.2057 lbs. avoirdupois.
Myriagramme, (10,000 grammes)	$321\frac{3}{8}$ oz. troy, or 22.057 lbs. avoirdupois.

Value of millimetres in English inches.

Millimetres.	English inches.	Millimetres.	English inches.	Millimetres.	English inches.
1	0.03937079	45	1.7716	125	4.941
2	0.07874158	50	1.968	130	5.118
3	0.11811237	55	2.165	135	5.315
4	0.15748316	60	2.362	140	5.512
5	0.19685395	65	2.559	145	5.708
6	0.23622474	70	2.756	150	5.906
7	0.27559553	75	2.953	155	6.103
8	0.31496632	80	3.149	160	6.299
9	0.35433711	85	3.346	165	6.496
10	0.39370790	90	3.543	170	6.693
15	0.5905	95	3.740	175	6.890
20	0.7874	100	3.937	180	7.087
25	0.9842	105	4.134	185	7.284
30	1.1811	110	4.331	190	7.480
35	1.3779	115	4.528	195	7.677
40	1.5748	120	4.744	200	7.874

Table for the conversion of degrees of Centigrade thermometers into those of Fahrenheit's scale.

Cent.	Fah.	Cent.	Fah.	Cent.	Fah.	Cent.	Fah.
—100	—148.0	—49	—56.2	2	35.6	53	127.4
— 99	—146.2	—48	—54.4	3	37.4	54	129.2
— 98	—144.4	—47	—52.6	4	39.2	55	131.0
— 97	—142.6	—46	—50.8	5	41.0	56	132.8
— 96	—140.8	—45	—49.0	6	42.8	57	134.6
— 95	—139.0	—44	—47.2	7	44.6	58	136.4
— 94	—137.2	—43	—45.4	8	46.4	59	138.2
— 93	—135.4	—42	—43.6	9	48.2	60	140.0
— 92	—133.6	—41	—41.8	10	50.0	61	141.8
— 91	—131.8	—40	—40.0	11	51.8	62	143.6
— 90	—130.0	—39	—38.2	12	53.6	63	145.4
— 89	—128.2	—38	—36.4	13	55.4	64	147.2
— 88	—126.4	—37	—34.6	14	57.2	65	149.0
— 87	—124.6	—36	—32.8	15	59.0	66	150.8
— 86	—122.8	—35	—31.0	16	60.8	67	152.6
— 85	—121.0	—34	—29.2	17	62.6	68	154.4
— 84	—119.2	—33	—27.4	18	64.4	69	156.2
— 83	—117.4	—32	—25.6	19	66.2	70	158.0
— 82	—115.6	—31	—23.8	20	68.0	71	159.8
— 81	—113.8	—30	—22.0	21	69.8	72	161.6
— 80	—112.0	—29	—20.2	22	71.6	73	163.4
— 79	—110.2	—28	—18.4	23	73.4	74	165.2
— 78	—108.4	—27	—16.6	24	75.2	75	167.0
— 77	—106.6	—26	—14.8	25	77.0	76	168.8
— 76	—104.8	—25	—13.0	26	78.8	77	170.6
— 75	—103.0	—24	—11.2	27	80.6	78	172.4
— 74	—101.2	—23	— 9.4	28	82.4	79	174.2
— 73	— 99.4	—22	— 7.6	29	84.2	80	176.0
— 72	— 97.6	—21	— 5.8	30	86.0	81	177.8
— 71	— 95.8	—20	— 4.0	31	87.8	82	179.6
— 70	— 94.0	—19	— 2.2	32	89.6	83	181.4
— 69	— 92.2	—18	— 0.4	33	91.4	84	183.2
— 68	— 90.4	—17	+ 1.4	34	93.2	85	185.0
— 67	— 88.6	—16	3.2	35	95.0	86	186.8
— 66	— 86.8	—15	5.0	36	96.8	87	188.6
— 65	— 85.0	—14	6.8	37	98.6	88	190.4
— 64	— 83.2	—13	8.6	38	100.4	89	192.2
— 63	— 81.4	—12	10.4	39	102.2	90	194.0
— 62	— 79.6	—11	12.2	40	104.0	91	195.8
— 61	— 77.8	—10	14.0	41	105.8	92	197.6
— 60	— 76.0	— 9	15.8	42	107.6	93	199.4
— 59	— 74.2	— 8	17.6	43	109.4	94	201.2
— 58	— 72.4	— 7	19.4	44	111.2	95	203.0
— 57	— 70.6	— 6	21.2	45	113.0	96	204.8
— 56	— 68.8	— 5	23.0	46	114.8	97	206.6
— 55	— 67.0	— 4	24.8	47	116.6	98	208.4
— 54	— 65.2	— 3	26.6	48	118.4	99	210.2
— 53	— 63.4	— 2	28.4	49	120.2	100	212.0
— 52	— 61.6	— 1	30.2	50	122.0	101	213.8
— 51	— 59.8	— 0	32.0	51	123.8	102	215.6
— 50	— 58.0	+ 1	33.8	52	125.6	103	217.4

The following table embraces the latest and most accurate equivalents, as accepted by Will, in the *Jahreslericht für Chemie*, for 1863:

Table of chemical equivalents of the sixty-three elements.

OXYGEN = 8.

Aluminum	Al	13.7	Norium	No	......
Antimony	Sb	122	Osmium	Os	99.6
Arsenic	As	75	Oxygen	O	8
Barium	Ba	68.5		~~O~~	16
Bismuth	Bi	210	Palladium	Pd	53.3
Boron	B	10.9	Phosphorus	P	31
Bromine	Br	80	Platinum	Pt	98.7
Carbon	C	6	Potassium	K	39
	~~C~~	12	Rhodium	Rh	52.2
Cadmium	Cd	50	Rubidium	Rb	85.4
Caesium	Cs	133	Ruthenium	Ru	52.2
Calcium	Ca	20	Sulphur	S	16
Cerium	Ce	43		~~S~~	32
Chlorine	Cl	35.5	Selenium	Se	39.7
Chromium	Cr	26.1	Silver	Ag	108
Cobalt	Co	29.4	Silicium	Si	14[3]
Copper	Cu	31.7			21[4]
Diarium	Di	......			28[5]
Didymium	D	47.5	Sodium	Na	23
Fluorine	Fl	19	Strontium	Sr	43.8
Gold	Au	197	Tantalum	Ta	68.8[6]
Glucinum	Gl	4.7[1]	Tellurium	Te	64
		7.0[2]	Thallium	Tl	204
Iodine	I	127		Th	59.16[7]
Iron	Fe	28			57.86[7]
Indium	In	......	Thorium	~~Th~~	118.3[8]
Iridium	Ir	99			115.7[8]
Lanthanum	La	46.4	Titanium	Ti	25
Lead	Pb	103.5	Uranium	U	60
Lithium	Li	7	Vanadium	V	68.6
Magnesium	Mg	12	Wasium (?)	Ws	......
Manganese	Mn	27.5	Wolfram	W	92[9]
Mercury	Hg	100		~~W~~	153.28[10]
	~~Hg~~	200	Yttrium	Y	......
Molybdenum	Mo	48	Zinc	Zn	32.6
Nickel	Ni	29.4		~~Zn~~	65
Niobium	Nb	48.8	Zirconium	Zr	22.4[11]
Nitrogen	N	14			33.6[12]
					44.8[13]

[1] If glucine = Gl O.
[2] If glucine = $Gl_2 O_3$.
[3] If silicic acid = Si O_2.
[4] If = Si O_3.
[5] If it = Si O_2.
[6] If tantalic acid = Ta O_2.
[7] If thoria = Th O.
[8] If it = Th O_2.
[9] If tungstic acid = Wo O_3.
[10] If it = ~~Wo~~ O_5.
[11] If zirconia = Zr O.
[12] If it = $Zr_2 O_3$.
[13] If it = Zr O_2.

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